YALE UNIVERSITY
MRS. HEPSA ELY SILLIMAN
MEMORIAL LECTURES

In the year 1883 a legacy of eighty thousand dollars was left to the President and Fellows of Yale College in the city of New Haven, to be held in trust, as a gift from her children, in memory of their beloved and honored mother, Mrs. Hepsa Ely Silliman.

On this foundation Yale College was requested and directed to establish an annual course of lectures designed to illustrate the presence and providence, the wisdom and goodness of God, as manifested in the natural and moral world. These were to be designated as the Mrs. Hepsa Ely Silliman Memorial Lectures. It was the belief of the testator that any orderly presentation of the facts of nature or history contributed to the end of this foundation more effectively than any attempt to emphasize the elements of doctrine or of creed; and he therefore provided that lectures on dogmatic or polemical theology should be excluded from the scope of this foundation, and that the subjects should be selected rather from the domains of natural science and history, giving special prominence to astronomy, chemistry, geology, and anatomy.

It was further directed that each annual course should be made the basis of a volume to form part of a series constituting a memorial to Mrs. Silliman. The memorial fund came into the possession of the Corporation of Yale University in the year 1901; and the present volume constitutes the fourteenth of the series of memorial lectures.

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A CENTURY OF SCIENCE
IN AMERICA

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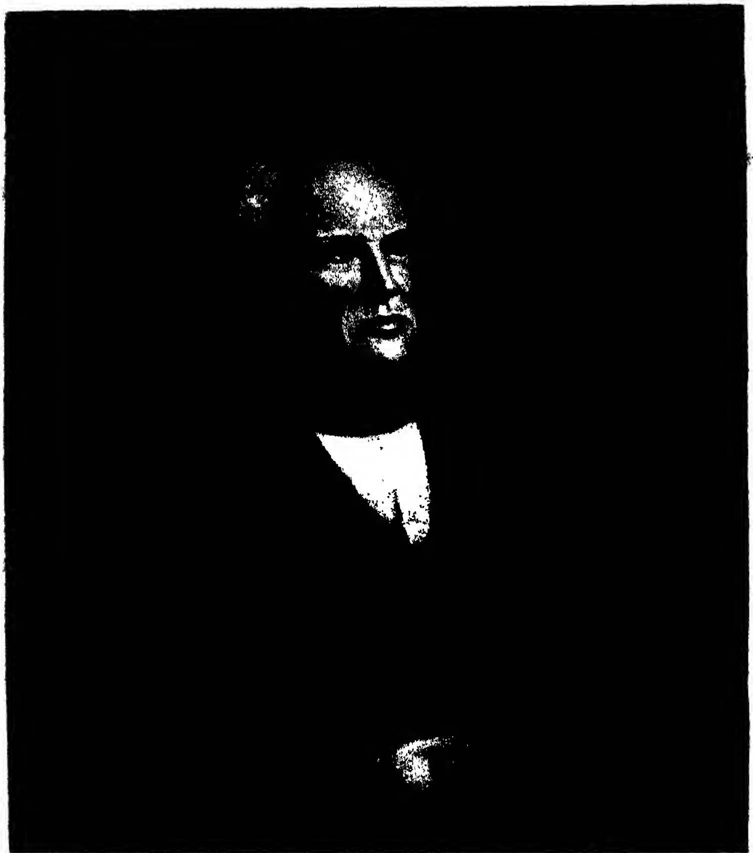

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B. Silliman

A
CENTURY OF SCIENCE
IN AMERICA

WITH SPECIAL REFERENCE TO THE AMERICAN JOURNAL OF SCIENCE
1818-1918

BY

EDWARD SALISBURY DANA · CHARLES SCHUCHERT
HERBERT E. GREGORY · JOSEPH BARRELL · GEORGE OTIS SMITH
RICHARD SWANN LULL · LOUIS V. PIRSSON
WILLIAM E. FORD · R. B. SOSMAN · HORACE L. WELLS
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PREFATORY NOTE

The present book commemorates the one-hundredth anniversary of the founding of the American Journal of Science by Benjamin Silliman in July, 1818. The opening chapter gives a somewhat detailed account of the early days of the Journal, with a sketch of its subsequent history. The remaining chapters are devoted to the principal branches of science which have been prominent in the pages of the Journal. They have been written with a view to showing in each case the position of the science in 1818 and the general progress made during the century; special prominence is given to American science and particularly to the contributions to it to be found in the Journal's pages. References to specific papers in the Journal are in most cases included in the text and give simply volume, page, and date, as (**24**, 105, 1833); when these and other references are in considerable number they have been brought together as a Bibliography at the end of the chapter.

The entire cost of the present book is defrayed from the income of the Mrs. Hepsa Ely Silliman Memorial Fund, established under the will of Augustus Ely Silliman, a nephew of Benjamin Silliman, who died in 1884. Certain of the chapters here printed have been made the basis of a series of seven Silliman Lectures in accordance with the terms of that gift. The selection of these lectures has been determined by the convenience of the gentlemen concerned and in part also by the nature of the subject.

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Benjamin Silliman................................*Frontispiece*

From a painting by U. D. Tenney, Esq., in possession of Miss Henrietta W. Hubbard

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A CENTURY OF SCIENCE IN AMERICA

I

THE AMERICAN JOURNAL OF SCIENCE FROM 1818 TO 1918

By EDWARD S. DANA

Introduction.

IN July, 1818, one hundred years ago, the first number of the American Journal of Science and Arts was given to the public. This is the only scientific periodical in this country to maintain an uninterrupted existence since that early date, and this honor is shared with hardly more than half a dozen other independent scientific periodicals in the world at large. Similar publications of learned societies for the same period are also very few in number.

It is interesting, on the occasion of this centenary, to glance back at the position of science and scientific literature in the world's intellectual life in the early part of the nineteenth century, and to consider briefly the marvelous record of combined scientific and industrial progress of the hundred years following—subjects to be handled in detail in the succeeding chapters. It is fitting also that we should recall the man who founded the Journal, the conditions under which he worked, and the difficulties he encountered. Finally, we must review, but more briefly, the subsequent history of what has so often been called after its founder, "Silliman's Journal."

The nineteenth century, and particularly the hundred years in which we are now interested, must always stand out in the history of the world as the period which has

combined the greatest development in all departments of science with the most extraordinary industrial progress. It was not until this century that scientific investigation used to their full extent the twin methods of observation and experiment. In cases too numerous to mention they have given us first, a tentative hypothesis; then, through the testing and correcting of the hypothesis by newly acquired data, an accepted theory has been arrived at; finally, by the same means carried further has been established one of nature's laws.

Early Science.—Looking far back into the past, it seems surprising that science should have had so late a growth, but the wonderful record of man's genius in the monuments he erected and in architectural remains shows that the working of the human mind found expression first in art and further man also turned to literature. So far as man's thought was constructive, the early results were systems of philosophy, and explanations of the order of things as seen from within, not as shown by nature herself. We date the real beginning of science with the Greeks, but it was the century that preceded Aristotle that saw the building of the Parthenon and the sculptures of Phidias. Even the great Aristotle himself (384-322 B. C.) though he is sometimes called the "founder of natural history," was justly accused by Lord Bacon many centuries later of having formed his theories first and then to have forced the facts to agree with them.

The bringing together of facts through observation alone began, to be sure, very early, for it was the motion of the sun, moon, and stars and the relation of the earth to them that first excited interest, and, especially in the countries of the East, led to the accumulation of data as to the motion of the planets, of comets and the occurrence of eclipses. But there was no coördination of these facts and they were so involved in man's superstition as to be of little value. In passing, however, it is worthy of mention that the Chinese astronomical data accumulated more than two thousand years before the Christian era have in trained hands yielded results of no small significance.

Doubtless were full knowledge available as to the

science existing in the early civilizations, we should rate it higher than we can at present, but it would probably prove even then to have been developed from within, like the philosophies of the Greeks, and with but minor influence from nature herself. It is indeed remarkable that down to the time with which we are immediately concerned, it was the branches of mathematics, as arithmetic and geometry and later their applications, that were first and most fully developed: in other words those lines of science least closely connected with nature.

Of the importance to science of the Greek school at Alexandria in the second and third centuries B. C., there can be no question. The geometry of Euclid (about 300 B. C.) was marvelous in its completeness as in clearness of logical method. Hipparchus (about 160-125 B. C.) gave the world the elements of trigonometry and developed astronomy so that Ptolemy 260 years later was able to construct a system that was well-developed, though in error in the fundamental idea as to the relative position of the earth. It is interesting to note that the Almagest of Ptolemy was thought worthy of republication by the Carnegie Institution only a year or two since. This great astronomical work, by the way, had no successor till that of the Arab Ulugh Bey in the fifteenth century, which within a few months has also been made available by the same Institution.

To the Alexandrian school also belongs Archimedes (287-212 B. C.), who, as every school boy knows, was the founder of mechanics and in fact almost a modern physical experimenter. He invented the water screw for raising water; he discovered the principle of the lever, which appealed so keenly to his imagination that he called for a ποῦ στῶ, or fulcrum, on which to place it so as to move the earth itself. He was still nearer to modern physics in his reputed plan of burning up a hostile fleet by converging the sun's rays by a system of great mirrors.

To the Romans, science owes little beyond what is implied in their vast architectural monuments, buildings and aqueducts which were erected at home and in the countries of their conquests. The elder Pliny (23-79 A. D.) most nearly deserved to be called a man of science,

but his work on natural history, comprised in thirty-seven volumes, is hardly more than a compilation of fable, fact, and fancy, and is sometimes termed a collection of anecdotes. He lost his life in the "grandest geological event of antiquity," the eruption of Vesuvius, which is vividly described by his nephew, the younger Pliny, in "one of the most remarkable literary productions in the domain of geology" (Zittel).

With the fall of Rome and the decline of Roman civilization came a period of intellectual darkness, from which the world did not emerge until the revival of learning in the fifteenth and sixteenth centuries. Then the extension of geographical knowledge went hand in hand with the development of art, literature, and the birth of a new science. Copernicus (1473-1543) gave the world at last a sun-controlled solar system; Kepler (1571-1630) formulated the laws governing the motion of the planets; Galileo (1564-1642) with his telescope opened up new vistas of astronomical knowledge and laid the foundations of mechanics; while Leonardo da Vinci (1452-1519), painter, sculptor, architect, engineer, musician and true scientist, studied the laws of falling bodies and solved the riddle of the fossils in the rocks. Still later Newton (1642-1727) established the law of gravitation, developed the calculus, put mechanics upon a solid basis and also worked out the properties of lenses and prisms so that his Optics (1704) will always have a prominent place in the history of science.

From the time of the Renaissance on science grew steadily, but it was not till the latter half of the eighteenth century that the foundations in most of the lines recognized to-day were fully laid. Much of what was accomplished then is, at least, outlined in the chapters following.

Our standpoint in the early years of the nineteenth century, just before the American Journal had its beginning, may be briefly summarized as follows: A desire for knowledge was almost universal and, therefore, also a general interest in the development of science. Mathematics was firmly established and the mathematical side of astronomy and natural philosophy—as physics was then called—was well developed. Many of the phenom-

ena of heat and their applications, as in the steam engine of Watt, were known and even the true nature of heat had been almost established by our countryman, Count Rumford; but of electricity there were only a few sparks of knowledge. Chemistry had had its foundation firmly laid by Priestley, Lavoisier, and Dalton, while Berzelius was pushing rapidly forward. Geology had also its roots down, chiefly through the work of Hutton and William Smith, though the earth was as yet essentially an unexplored field. Systematic zoology and botany had been firmly grounded by Buffon, Lamarck and Cuvier, on the one hand, and Linnæus on the other; but of all that is embraced under the biology of the latter half of the nineteenth century the world knew nothing. The statements of Silliman in his Introductory Remarks in the first number, quoted in part on a following page, put the matter still more fully, but they are influenced by the enthusiasm of the time and he could have had little comprehension of what was to be the record of the next one hundred years.

Now, leaving this hasty and incomplete retrospect and coming down to 1918, we find the contrast between to-day and 1818 perhaps most strikingly brought out, on the material side, if we consider the ability of man, in the early part of the nineteenth century, to meet the demands upon him in the matter of transportation of himself and his property. In 1800, he had hardly advanced beyond his ancestor of the earliest civilization; on the contrary, he was still dependent for transportation on land upon the muscular efforts of himself and domesticated animals, while at sea he had only the use of sails in addition. The first application of the steam engine with commercial success was made by Fulton when, in 1807, the steamboat "Clermont" made its famous trip on the Hudson River. Since then, step by step, transportation has been made more and more rapid, economical and convenient, both on land and water. This has come first through the perfection of the steam engine; later through the agency of electricity, and still further and more universally by the use of gasolene motors. Finally, in these early years of the twentieth century, what seemed once a wild dream of

the imagination has been realized, and man has gained the conquest of the air; while the perfection of the submarine is as wonderful as its work can be deadly.

Hardly less marvelous is the practical annihilation of space and time in the electric transmission of human thought and speech by wire and by ether waves. While, still further, the same electrical current now gives man his artificial illumination and serves him in a thousand ways besides.

But the limitations of space have also been conquered, during the same period, by the spectroscope which brings a knowledge of the material nature of the sun and the fixed stars and of their motion in the line of sight; while spectrum analysis has revealed the existence of many new elements and opened up vistas as to the nature of matter.

The chemist and the physicist, often working together in the investigation of the problems lying between their two departments, have accumulated a staggering array of new facts from which the principles of their sciences have been deduced. Many new elements have been discovered, in fact nearly all called for by the periodic law; the so-called fixed gases have been liquefied, and now air in liquid form is almost a plaything; the absolute zero has been nearly reached in the boiling point of helium; physical measurements in great precision have been carried out in both directions for temperatures far beyond any scale that was early conceived possible; the atom, once supposed to be indivisible, has been shown to be made up of the much smaller electrons, while its disintegration in radium and its derivatives has been traced out and with consequences only as yet partly understood but certainly having far-reaching consequences; at one point we seem to be brought near to the transmutation of the elements which was so long the dream of the alchemist. Still again photography has been discovered and perfected and with the use of X-rays it gives a picture of the structure of bodies totally opaque to the eye; the same X-rays seem likely to locate and determine the atoms in the crystal.

Here and at many other points we are reaching out to a knowledge of the ultimate nature of matter.

In geology, vast progress has been made in the knowledge of the earth, not only as to its features now exhibited at or near the surface, but also as to its history in past ages, of the development of its structure, the minute history of its life, the phenomena of its earthquakes, volcanoes, etc. Geological surveys in all civilized countries have been carried to a high degree of perfection.

In biology, itself a word which though used by Lamarck did not come into use till taken up by Huxley, and then by Herbert Spencer in the middle of the century, the progress is no less remarkable as is well developed in a later chapter of this volume.

Although not falling within our sphere, it would be wrong, too, not to recognize also the growth of medicine, especially through the knowledge of bacteria and their functions, and of disease germs and the methods of combating them. The world can never forget the debt it owes to Pasteur and Lister and many later investigators in this field.

To follow out this subject further would be to encroach upon the field of the chapters following, but, more important and fundamental still than all the facts discovered and the phenomena investigated has been the establishment of certain broad scientific principles which have revolutionized modern thought and shown the relation between sciences seemingly independent. The law of conservation of energy in the physical world and the principle of material and organic evolution may well be said to be the greatest generalizations of the human mind. Although suggestions in regard to them, particularly the latter, are to be found in the writings of early authors, the establishment and general acceptance of these principles belong properly to the middle of the nineteenth century. They stand as the crowning achievement of the scientific thought of the period in which we are interested.

Any mere enumeration of the vast fund of knowledge accumulated by the efforts of man through observation and experiment in the period in which we are interested would be a dry summary, and yet would give some measure of what this marvelous period has accomplished. As

in geography, man's energy has in recent years removed the reproach of a "Dark Continent," of "unexplored" central Asia and the once "inaccessible polar regions," so in the different departments of science, he has opened up many unknown fields and accumulated vast stores of knowledge. It might even seem as if the limit of the unknown were being approached. There remains, however, this difference in the analogy, that in science the fundamental relations—as, for example, the nature of gravitation, of matter, of energy, of electricity; the actual nature and source of life—the solution of these and other similar problems still lies in the future. What the result of continued research may be no one can predict, but even with these possibilities before us, it is hardly rash to say that so great a combined progress of pure and applied science as that of the past hundred years is not likely to be again realized.

Scientific Periodical Literature in 1818.

The contrast in scientific activity between 1818 and 1918 is nowhere more strikingly shown than in the amount of scientific periodical literature of the two periods. Of the thousands of scientific journals and regular publications by scientific societies and academies to-day, but a very small number have carried on a continuous and practically unbroken existence since 1818. This small amount of periodical scientific literature in the early part of the last century is significant as giving a fair indication of the very limited extent to which scientific investigation appealed to the intellectual life of the time. Some definite facts in regard to the scientific publications of those early days seem to be called for.

Learned societies and academies, devoted to literature and science, were formed very early but at first for occasional meetings only and regular publications were in most cases not begun till a very much later date. Some of the earliest—not to go back of the Renaissance—are the following:

1560. Naples, Academia Secretorum Naturæ.
1603. Rome, Accademia dei Lincei.
1651. Leipzig, Academia Naturæ Curiosum.

1657. Florence, Accademia del Cimento.
1662. London, Royal Society.
1666. Paris, Académie des Sciences.
1690. Bologna, Accademia delle Scienze.
1700. Berlin, Societas Regia Scientiarum. This was the forerunner of the K. preuss. Akad. d. Wissenschaften.

The Royal Society of London, whose existence dates from 1645, though not definitely chartered until 1662, began the publication of its "Philosophical Transactions" in 1665 and has continued it practically unbroken to the present time; this is a unique record. Following this, other early—but in most cases not continuous—publications were those of Paris (1699); Berlin (1710); Upsala (1720); Petrograd, 1728; Stockholm (1739); and Copenhagen (1743).

For the latter half of the eighteenth century, when the foundations of our modern science were being rapidly laid, a considerable list might be given of early publications of similar scientific bodies. Some of the prominent ones are: Göttingen (1750), Munich (1759), Brussels (1769), Prague (1775), Turin (1784), Dublin (1788), etc. The early years of the nineteenth century saw the beginnings of many others, particularly in northern Italy. It is to be noted that, as stated, only rarely were the publications of these learned societies even approximately continuous. In the majority of cases the issue of transactions or proceedings was highly irregular and often interrupted.

In this country the earliest scientific bodies are the following:

Philadelphia. American Philosophical Society, founded in 1743. Transactions were published 1771-1809; then interrupted until 1818 *et seq.*

Boston. American Academy of Arts and Sciences, founded in 1780. Memoirs, 1785-1821; and then 1833 *et seq.*

New Haven. Connecticut Academy of Arts and Sciences, begun in 1799. Memoirs, vol. 1, 1810-16; Transactions, 1866 *et seq.*

Philadelphia. Academy of Natural Sciences, begun in 1812. Journal, 1817-1842; and from 1847 *et seq.*

New York. Lyceum of Natural History, 1817; later (1876) became the New York Academy of Sciences. Annals from 1823; Proceedings from 1870.

The situation is somewhat similar as to independent scientific journals. A list of the names of those started only to find an early death would be a very long one, but interesting only historically and as showing a spasmodic but unsustained striving after scientific growth.

It seems worth while, however, to give here the names of the periodicals embracing one or more of the subjects of the American Journal, which began at a very early date and most of which have maintained an uninterrupted existence down to 1915. It should be added that certain medical journals, not listed here, have also had a long and continued existence.[1]

Early Scientific Journals.

1771–1823. Journal de Physique, Paris; title changed several times.

1787–. Botanical Magazine. (For a time known as Curtis's Journal.)

1789–1816. Annales de Chimie, Paris. Continued from 1817 on as the Annales de Chimie et de Physique.

1790. Journal der Physik, Halle (by Gren); from 1799 on became the Annalen der Physik (und Chemie), Halle, Leipzig. The title has been somewhat changed from time to time though publication has been continuous. Often referred to by the name of the editor-in-chief, as Gren, Gilbert, Poggendorff, Wiedemann, etc.

1795–1815. Journal des Mines, Paris, continued from 1816 as the Annales des Mines.

1796–1815. Bibliothèque Britannique, Geneva. From 1816–1840, Bibliothèque Universelle, etc. 1846–1857, Archives des Sci. phys. nat. Since 1858 generally known as the Bibliothèque Universelle.

1797. Journal of Natural Philosophy, Chemistry and the Arts (Nicholson's Journal) London; united in 1814 with the Philosophical Magazine (Tilloch's Journal).

1798–. The Philosophical Magazine (originally by Tilloch). This absorbed Nicholson's Journal (above) in 1814; also the Annals of Philosophy (Thomson, Phillips) in 1827 and Brewsters' Edinburgh Journal of Science in 1832.

1798–1803. Allgemeines Journal de Chemie (Scherer's Journal). 1803–1806; continued as Neues Allg. J. etc. (Gehlen's Journal.) Later title repeatedly changed and finally (1834 *et seq.*) Journal für praktische Chemie.

1816–18. Journal of Science and the Arts, London. 1819–

30, Quarterly J. etc. 1830–31, Journal of the Royal Institution of Great Britain.

1818. American Journal of Science and Arts until 1880, when "the Arts" was dropped, New Haven, Conn. First Series, **1-50**, 1818–1845; Second Series, **1-50**, 1846–1870; Third Series, **1-50**, 1871-1895; Fourth Series, **1-45**, 1896–June, 1918.

1818. Flora, or Allgemeine botanische Zeitung. Regensburg, Munich.

1820–1867. London Journal of Arts and Sciences (after 1855, Newton's Journal).

1824–. Annales des sciences naturelles. Paris.

1826–. Linnæa, Berlin, Halle; from 1882 united with Jahrb. d. K. botan. Gartens.

1828–1840. Magazine of Natural History, London; united 1838 with the Annals of Natural History, and known since 1841 as the Annals and Magazine of Natural History.

1828–. Journal of the Franklin Institute Philadelphia, from 1826; earlier (1825) the American Mechanics Magazine.

1832–. Annalen der Chemie (und Pharmacie) often known as Liebig's Annalen. Leipzig, Lemgo.

The Founder of the American Journal of Science.

The establishment of a scientific journal in this country in 1818 was a pioneer undertaking, requiring of its founder a rare degree of energy, courage, and confidence in the future. It was necessary, not only to obtain the material to fill its pages and the money to carry on the enterprise, but, before the latter end could be accomplished, an audience must be found among those who had hitherto felt little or no interest in the sciences. This great work was accomplished by Benjamin Silliman, "the guardian of American Science," whose influence was second to none in the early development of science in this country. Before speaking in some detail of the early years of this Journal and of its subsequent history, it is proper that some words should be given to its founder.

Benjamin Silliman, son of a general prominent in the Revolutionary War, was born in Trumbull, Connecticut, on August 8, 1779. He was a graduate of Yale College of the class of 1796. Though at first a student of law and accepted for the bar in Connecticut, he was called in 1802 by President Timothy Dwight—a man of rare breadth of

mind—to occupy the newly-made chair of chemistry, mineralogy (and later geology) in Yale College at New Haven. To fit himself for the work before him he carried on extensive studies at home and in Philadelphia and spent the year 1805 in travels and study at London and Edinburgh, and also on the Continent. His active duties began in 1806 and from this time on he was in the service of Yale College until his resignation in 1853. From the first, Silliman met with remarkable success as a teacher and public lecturer in arousing an interest in science. His breadth of knowledge, his enthusiasm for his chosen subjects and power of clear presentation, combined with his fine presence and attractive personality, made him a great leader in the science of the country and gave him a unique position in the history of its development.

Much might be said of the man and his work, but, the best tribute is that of James Dwight Dana, given in his inaugural address upon the occasion of his beginning his duties as Silliman professor of geology in Yale College. This was delivered on February 18, 1856, in what was then known as the "Cabinet Building." Dana says in part:

"In entering upon the duties of this place, my thoughts turn rather to the past than to the subject of the present hour. I feel that it is an honored place, honored by the labors of one who has been the guardian of American Science from its childhood; who here first opened to the country the wonderful records of geology; whose words of eloquence and earnest truth were but the overflow of a soul full of noble sentiments and warm sympathies, the whole throwing a peculiar charm over his learning, and rendering his name beloved as well as illustrious. Just fifty years since, Professor Silliman took his station at the head of chemical and geological science in this college. Geology was then hardly known by name in the land, out of these walls. Two years before, previous to his tour in Europe, the whole cabinet of Yale was a half-bushel of unlabelled stones. On visiting England he found even in London no school public or private, for geological instruction, and the science was not named in the English universities. To the mines, quarries, and cliffs of England, the crags of Scotland, and the meadows of Holland he looked for knowledge, and from these and the teachings of Murray, Jameson, Hall, Hope, and Playfair, at Edinburgh, Professor Silliman returned, equipped for duty,—albeit

a great duty,—that of laying the foundation, and creating almost out of nothing a department not before recognized in any institution in America.

He began his work in 1806. The science was without books—and, too, without system, except such as its few cultivators had each for himself in his conceptions. It was the age of the first beginnings of geology, when Wernerians and Huttonians were arrayed in a contest. . . . Professor Silliman when at Edinburgh witnessed the strife, and while, as he says, his earliest predilections were for the more peaceful mode of rock-making, these soon yielded to the accumulating evidence, and both views became combined in his mind in one harmonious whole. The science, thus evolved, grew with him and by him; for his own labors contributed to its extension. Every year was a year of expansion and onward development, and the grandeur of the opening views found in him a ready and appreciative response.

. . .

And while the sciences and truth have thus made progress here, through these labors of fifty years, the means of study in the institution have no less increased. Instead of that half-bushel of stones, which once went to Philadelphia for names, in a candle-box, you see above the largest mineral cabinet in the country, which but for Professor Silliman, his attractions and his personal exertions together, would never have been one of the glories of old Yale. . . .

Moreover, the American Journal of Science,—now in its thirty-seventh year and seventieth volume [1856],—projected and long-sustained solely by Professor Silliman, while ever distributing truth, has also been ever gathering honors, and is one of the laurels of Yale.

We rejoice that in laying aside his studies, after so many years of labor, there is still no abated vigor. . . . He retires as one whose right it is to throw the burden on others. Long may he be with us, to enjoy the good he has done, and cheer us by his noble and benign presence."

In addition to these words of Dana, much of vital interest in regard to Silliman and his work will be gathered from what is given in the pages immediately following, quoted from his personal statements in the early volumes of the Journal.

The Early Years of the Journal.

In no direction did Silliman's enthusiastic activities in science produce a more enduring result than in the found-

THE AMERICAN JOURNAL OF SCIENCE,

MORE ESPECIALLY OF

MINERALOGY, GEOLOGY,

AND THE

OTHER BRANCHES OF NATURAL HISTORY;

INCLUDING ALSO

AGRICULTURE

AND THE

ORNAMENTAL AS WELL AS USEFUL

ARTS.

CONDUCTED BY

BENJAMIN SILLIMAN,

PROFESSOR OF CHEMISTRY, MINERALOGY, ETC IN YALE COLLEGE, AUTHOR OF TRAVELS IN ENGLAND, SCOTLAND, AND HOLLAND, ETC

VOL. I.....NO. I.

ENGRAVING IN THE PRESENT NO.

New apparatus for the combustion of TAR, &c. by the vapour of water.

New-York:

PUBLISHED BY J. EASTBURN AND CO. LITERARY ROOMS, BROADWAY, AND BY HOWE AND SPALDING, NEW-HAVEN

Abraham Paul, printer.

1818.

ing and carrying on of the Journal. The first suggestion in regard to the enterprise was made to Silliman by his friend, Colonel George Gibbs, from whom the famous Gibbs collection of minerals was bought by Yale College in 1825. Silliman says (**25**, 215, 1834):

"Col. Gibbs was the person who first suggested to the Editor the project of this Journal, and he urged the topic with so much zeal and with such cogent arguments, as prevailed to induce the effort in a case then viewed as of very dubious success. The subject was thus started in November, 1817; proposals for the Journal were issued in January, 1818, and the first number appeared in July of that year."

He adds further (**50**, p. iii, 1847) that the conversation here recorded took place "on an accidental meeting on board the steamboat Fulton in Long Island Sound." This was some ten years after Robert Fulton's steamboat, the Clermont, made its pioneer trip on the Hudson river, already alluded to. The incident is not without significance in this connection. The deck of the "Fulton" was not an inappropriate place for the inauguration of an enterprise also great in its results for the country.

In the preface to the concluding volume of the First Series (loc. cit.) Silliman adds the following remarks which show his natural modesty at the thought of undertaking so serious a work. He says:

Although a different selection of an editor would have been much preferred, and many reasons, public and personal, concurred to produce diffidence of success, the arguments of Col. Gibbs, whose views on subjects of science were entitled to the most respectful consideration, and had justly great weight, being pressed with zeal and ability, induced a reluctant assent; and accordingly, after due consultation with many competent judges, the proposals were issued early in 1818, embracing the whole range of physical science and its applications. The Editor in entering on the duty, regarded it as an affair for life, and the thirty years of experience which he has now had, have proved that his views of the exigencies of the service were not erroneous.

The plan with which the editor began his work and the lines laid down by him at the outset can only be made clear by quoting entire the "Plan of the Work" which

opens the first number. It seems desirable also to give this in its original form as to paragraphs and typography. The first page of the cover of the opening number has also been reproduced here. It will be seen that the plan of the young editor was as wide as the entire range of science and its applications and extended out to music and the fine arts. This seems strange to-day, but it must be remembered how few were the organs of publication open tc contributors at the time. If the plan was unreasonably extended, that fact is to be taken not only as an expression of the enthusiasm of the editor, as yet inexperienced in his work, but also of the time when the sciences were still in their infancy.

He says (**1**, pp. v, vi):

"PLAN OF THE WORK.

This Journal is intended to embrace the circle of THE PHYSICAL SCIENCES, with their application to THE ARTS, and to every useful purpose.

It is designed as a deposit for *original American communications;* but will contain also occasional selections from Foreign Journals, and notices of the progress of science in other countries. Within its plan are embraced

NATURAL HISTORY, in its three great departments of MINERALOGY, BOTANY, and ZOOLOGY;

CHEMISTRY and NATURAL PHILOSOPHY, in their various branches: and MATHEMATICS, pure and mixed.

It will be a leading object to illustrate AMERICAN NATURAL HISTORY, and especially our MINERALOGY and GEOLOGY.

The APPLICATIONS of these sciences are obviously as numerous as *physical arts,* and *physical wants;* for no one of these arts or wants can be named which is not connected with them.

While SCIENCE will be cherished *for its own sake,* and with a due respect for its own *inherent* dignity; it will also be employed as the *handmaid to the Arts.* Its numerous applications to AGRICULTURE, the earliest and most important of them; to our MANUFACTURES, both mechanical and chemical; and to our DOMESTIC ECONOMY, will be carefully sought out, and faithfully made.

It is also within the design of this Journal to receive communications on MUSIC, SCULPTURE, ENGRAVING, PAINTING, and generally on the fine and liberal, as well as useful arts;

On Military and Civil Engineering, and the art of Navigation.

Very truly Yours, B. Silliman

Notices, Reviews, and Analyses of new scientific works, and of new Inventions, and Specifications of Patents;

Biographical and Obituary Notices of scientific men; essays on COMPARATIVE ANATOMY and PHYSIOLOGY, and generally on such other branches of medicine as depend on scientific principles;

Meteorological Registers, and Reports of Agricultural Experiments: and we would leave room also for interesting miscellaneous things, not perhaps exactly included under either of the above heads.

Communications are respectfully solicited from men of science, and *from men versed in the practical arts.*

Learned Societies are invited to make this Journal, occasionally, the vehicle of their communications to the Public.

The editor will not hold himself responsible for the sentiments and opinions advanced by his correspondents; but he will consider it as an allowed liberty to make slight *verbal alterations,* where errors may be presumed to have arisen from inadvertency."

In the "Advertisement" which precedes the above statement in the first number, the editor remarks somewhat naïvely that he "does not pledge himself that all the subjects shall be touched upon in every number. This is plainly impossible unless every article should be very short and imperfect. . ."

The whole subject is discussed in all its relations in the "Introductory Remarks" which open the first volume. No apology is needed for quoting at considerable length, for only in this way can the situation be made clear, as seen by the editor in 1818. Further we gain here a picture of the intellectual life of the times and, not less interesting, of the mind and personality of the writer. With a frank kindliness, eminently characteristic of the man, as will be seen, he takes the public fully into his confidence. In the remarks made in subsequent volumes,—also extensively quoted—the vicissitudes in the conduct of the enterprise are brought out and when success was no longer doubtful, there is a tone of quiet satisfaction which was also characteristic and which the circumstances fully justified.

The INTRODUCTORY REMARKS begin as follows:

The age in which we live is not less distinguished by a vigorous and successful cultivation of physical science, than by its numer-

ous and important applications to the practical arts, and to the common purposes of life.

In every enlightened country, men illustrious for talent, worth and knowledge, are ardently engaged in enlarging the boundaries of natural science; and the history of their labors and discoveries is communicated to the world chiefly through the medium of scientific journals. The utility of such journals has thus become generally evident; they are the heralds of science; they proclaim its toils and its achievements; they demonstrate its intimate connection as well with the comfort, as with the intellectual and moral improvement of our species; and they often procure for it enviable honors and substantial rewards.

Mention is then made of the journals existing in England and France in 1818 "which have long enjoyed a high and deserved reputation." He then continues:

From these sources our country reaps and will long continue to reap, an abundant harvest of information: and if the light of science, as well as of day, springs from the East, we will welcome the rays of both; nor should national pride induce us to reject so rich an offering.

But can we do nothing in return?

In a general diffusion of useful information through the various classes of society, in activity of intellect and fertility of resource and invention, producing a highly intelligent population, we have no reason to shrink from a comparison with any country. But the devoted cultivators of science in the United States are comparatively few: they are, however, rapidly increasing in number. Among them are persons distinguished for their capacity and attainments, and, notwithstanding the local feelings nourished by our state sovereignties, and the rival claims of several of our larger cities, there is evidently a predisposition towards a concentration of effort, from which we may hope for the happiest results, with regard to the advancement of both the science and reputation of our country.

Is it not, therefore, desirable to furnish some rallying point, some object sufficiently interesting to be nurtured by common efforts, and thus to become the basis of an enduring, common interest? To produce these efforts, and to excite this interest, nothing, perhaps, bids fairer than a SCIENTIFIC JOURNAL.

The valuable work already accomplished by various medical journals is then spoken of and particularly that of the first scientific periodical in the United States, Bruce's Mineralogical Journal. This, as Silliman says

(1, p. 3, 1818), although "both in this country and in Europe received in a very flattering manner," did not survive the death of its founder, and only a single volume of 270 pages appeared (1810-1813).

Silliman continues:

No one, it is presumed, will doubt that a journal devoted to science, and embracing a sphere sufficiently extensive to allure to its support the principal scientific men of our country, is greatly needed; if cordially supported, it will be successful, and if successful, it will be a great public benefit.

Even a failure, in so good a cause, (unless it should arise from incapacity or unfaithfulness,) cannot be regarded as dishonourable. It may prove only that the attempt was premature, and that our country is not yet ripe for such an undertaking; for without the efficient support of talent, knowledge, and money, it cannot long proceed. No editor can hope to carry forward such a work without the active aid of scientific and practical men; but, at the same time, the public have a right to expect that he will not be sparing of his own labour, and that his work shall be generally marked by the impress of his own hand. To this extent the editor cheerfully acknowledges his obligations to the public; and it will be his endeavour faithfully to redeem his pledge.

Most of the periodical works of our country have been short-lived. This, also, may perish in its infancy; and if any degree of confidence is cherished that it will attain a maturer age, it is derived from the obvious and intrinsic importance of the undertaking; from its being built upon permanent and momentous national interests; from the evidence of a decided approbation of the design, on the part of gentlemen of the first eminence, obtained in the progress of an extensive correspondence; from assurance of support, in the way of contributions, from men of ability in many sections of the union; and from the existence of such a crisis in the affairs of this country and of the world, as appears peculiarly auspicious to the success of every wise and good undertaking.

An interesting discussion follows (pp. 5-8) as to the claims of the different branches of science, and the extent to which they and their applications had been already developed, also the spheres still open to discovery.

The Introductory Remarks close, as follows:

In a word, the whole circle of physical science is directly applicable to human wants and constantly holds out a light to

the practical arts; it thus polishes and benefits society and everywhere demonstrates both supreme intelligence and harmony and beneficence of design in the Creator.

The science of mathematics, both pure and mixed, can never cease to be interesting and important to man, as long as the relations of quantity shall exist, as long as ships shall traverse the ocean, as long as man shall measure the surface or heights of the earth on which he lives, or calculate the distances and examine the relations of the planets and stars; and as long as the *iron reign of war* shall demand the discharge of projectiles, or the construction of complicated defences.

The closing part of the paragraph shows the influence exerted upon the mind of the editor by the serious wars of the years preceding 1818, a subject alluded to again at the close of this chapter.

In February, 1822, with the completion of the fourth volume, the editor reviews the situation which, though encouraging is by no means fully assuring. He says (preface to vol. 4, dated Feb. 15, 1822):

Two years and a half have elapsed, since the publication of the first volume of this Journal, and one year and ten months since the Editor assumed the pecuniary responsibility. . . .

The work has not, even yet, reimbursed its expenses, (we speak not of editorial or of business compensation,) we intend, that it has not paid for the paper, printing and engraving; the proprietors of the first volume being in advance, on those accounts, and the Editor on the same score, with respect to the aggregate expense of the three last volumes. This deficit is, however, no longer increasing, as the receipts, at present, just about cover the expense of the physical materials, and of the manual labour. A reiterated disclosure of this kind is not grateful, and would scarcely be manly, were it not that the public, who alone have the power to remove the difficulty, have a right to a frank exposition of the state of the case. As the patronage is, however, growing gradually more extensive, it is believed that the work will be eventually sustained, although it may be long before it will command any thing but gratuitous intellectual labour. . . .

These facts, with the obvious one,—that its pages are supplied with contributions from all parts of the Union, and occasionally from Europe, evince that the work is received as a national and not as a local undertaking, and that the community consider it as having no sectional character. Encouraged by this view of

the subject, and by the favour of many distinguished men, both at home and abroad, and supported by able contributors, to whom the Editor again tenders his grateful acknowledgments, he will still persevere, in the hope of contributing something to the advancement of our science and arts, and towards the elevation of our national character.

In the autumn of the same year, the editor closes the fifth volume with a more confident tone (Sept. 25, 1822):

A trial of four years has decided the point, that the American Public will support this Journal. Its pecuniary patronage is now such, that although not a lucrative, it is no longer a hazardous enterprise. It is now also decided, that the intellectual resources of the country are sufficient to afford an unfailing supply of valuable original communications and that nothing but perseverance and effort are necessary to give perpetuity to the undertaking.

The decided and uniform expression of public favour which the Journal has received both at home and abroad, affords the Editor such encouragement, that he cannot hesitate to persevere—and he now renews the expression of his thanks to the friends and correspondents of the work, both in Europe and the United States, requesting at the same time a continuance of their friendly influence and efforts.

Still again in the preface to the sixth volume (1823) he takes the reader more fully into his confidence and shows that he regards the enterprise as no longer of doubtful success. He says:

The conclusion of a new volume of a work, involving so much care, labour and responsibility, as are necessarily attached, at the present day, to a Journal of Science and the Arts, naturally produces in the mind, a state of not ungrateful calmness, and a disposition, partaking of social feeling, to say something to those who honour such a production, by giving to it a small share of their money, and of their time. The Editor's first impression was, that the sixth volume should be sent into the world without an introductory note, but he yields to the impulse already expressed, and to the established usages of respectful courtesy to the public, which a short preface seems to imply. He has now persevered almost five years, in an undertaking, regarded by many of the friends whom he originally consulted, as hazardous, and to which not a few of them prophetically alloted only an ephemeral existence. It has been his fortune to

prosecute this work without, (till a very recent period,) returns, adequate to its indispensable responsibilities;—under a heavy *pressure of professional and private duty; with trying fluctuations of health, and amidst severe and reiterated domestic* afflictions. The world are usually indulgent to allusions of this nature, when they have any relation to the discharge of public duty; and in this view, it is with satisfaction, that the Editor adds, that he has now to look on formidable difficulties, only in retrospect, and with something of the feeling of him, who sees a powerful and vanquished foe, slowly retiring, and leaving a field no longer contested.

This Journal which, from the first, was fully supplied with original communications, is now sustained by actual payment, to such an extent, that it may fairly be considered as an established work; its patronage is regularly increasing, and we trust it will no longer justify such remarks as some of the following, from the pen of one of the most eminent scientific men in Europe. "Nothing surprises me more, than the little encouragement which your Journal," ("which I always read with very great interest, and of which I make great use,") "experiences in America—this must surely arise from the present depressed condition of trade, and cannot long continue."

Six years more of uninterrupted editorial work passed by, the sixteenth volume was completed, and the editor was now in a position to review the whole situation up to 1829. This preface (dated July 1, 1829), which is quoted nearly in full, cannot fail to be found particularly interesting and from several standpoints, not the least for the insight it gives into the writer's mind. It is also noteworthy that at this early date it was found possible to pay for original contributions, a privilege far beyond the means of the editor of to-day.

When this Journal was first projected, very few believed that it would succeed.

Among others, Dr. Dorsey wrote to the editor; "I predict a short life for you, although I wish, as the Spaniards say, that you may live a thousand years." The work has not lived a thousand years, but as it has survived more than the hundredth part of that period, no reason is apparent why it may not continue to exist. To the contributors, disinterested and arduous as have been their exertions, the editor's warmest thanks are due; and they are equally rendered to numerous personal friends for their unwavering support: nor ought those sub-

scribers to be forgotten who, occupied in the common pursuits of life, have aided, by their money, in sustaining the hazardous novelty of an American Journal of Science. A general approbation, sufficiently decided to encourage effort, where there was no other reward, has supported the editor; but he has not been inattentive to the voice of criticism, whether it has reached him in the tones of candor and kindness, or in those of severity. We must not look to our friends for the full picture of our faults. He is unwise who neglects the maxim—

—fas est ab hoste doceri,

and we may be sure, that those are quite in earnest, whose pleasure it is, to place faults in a strong light and bold relief; and to throw excellencies into the shadow of total eclipse. Minds at once enlightened and amiable, viewing both in their proper proportions, will however render the equitable verdict;

Non ego paucis offendar maculis,—

It is not pretended that this Journal has been faultless; there may be communications in it which had been better omitted, and it is not doubted that the power to command intellectual effort, by suitable pecuniary reward, would add to its purity, as a record of science, and to its richness, as a repository of discoveries in the arts.

But the editor, even now, offers payment, at the rate adopted by the literary Journals, for able original communications, containing especially important facts, investigations and discoveries in science, and practical inventions in the useful and ornamental Arts.

As however his means are insufficient to pay for all the copy, it is earnestly requested, that those gentlemen, who, from other motives, are still willing to write for this Journal, should continue to favor it with their communications. That the period when satisfactory compensation can be made to all writers whose pieces are inserted, and to whom payment will be acceptable, is not distant, may perhaps be hoped, from the spontaneous expression of the following opinion, by the distinguished editor of one of our principal literary journals, whose letter is now before me. "The character of the American Journal is strictly national, and it is the only vehicle of communication in which an inquirer may be sure to find what is most interesting in the wide range of topics, which its design embraces. It has become in short, not more identified with the science than the literature of the country." It is believed that a strict examination of its contents will prove that its character has been decidedly scientific; and the opinion is often expressed to the editor, that

in common with the journals of our Academies, it is a work of reference, indispensable to him who would examine the progress of American science during the period which it covers. That it might not be too repulsive to the general reader, some miscellaneous pieces have occasionally occupied its pages; but in smaller proportion, than is common with several of the most distinguished British Journals of Science.

Still, the editor has been frequently solicited, both in public and private, to make it more miscellaneous, that it might be more acceptable to the intelligent and well educated man, who does not cultivate science; but he has never lost sight of his great object, which was to produce and concentrate original American effort in science, and thus he has foregone pecuniary returns, which by pursuing the other course, might have been rendered important. Others would not have him admit any thing that is not strictly and technically scientific; and would make this journal for mere professors and amateurs; especially in regard to those numerous details in natural history, which although important to be registered, (and which, when presented, have always been recorded in the American Journal,) can never exclusively occupy the pages of any such work without repelling the majority of readers.

If this is true even in Great Britain it is still more so in this country; and our savants, unless they would be, not only the exclusive admirers, but the sole purchasers of their own works, must permit a little of the graceful drapery of general literature to flow around the cold statues of science. The editor of this Journal, strongly inclined, both from opinion and habit, to gratify the cultivators of science, will still do everything in his power to promote its high interests, and as he hopes in a better manner than heretofore; but these respectable gentlemen will have the courtesy, to yield something to the reading literary, as well as scientific public, and will not, we trust, be disgusted, if now and then an *Oasis* relieves the eye, and a living stream refreshes the traveller. Not being inclined to renew the abortive experiment, to please every body, which has been so long renowned in fable; the editor will endeavor to pursue, the even tenor of his way; altogther inclined to be courteous and useful to his fellow travellers, and hoping for their kindness and services in return.

The Close of the First Series.

The "First Series," as it was henceforth to be known, closed with the fiftieth volume (1847, pp. xx + 347). This final volume is devoted to an exhaustive index to the

James D. Dana

forty-nine volumes preceding. In the preface (dated April 19, 1847) the elder Silliman, now the senior editor, reviews the work that had been accomplished with a frank expression of his feeling of satisfaction in the victory won against great obstacles; with this every reader must sympathize. He quotes here at length (but in slightly altered form) the matter from the first volume (1818), which has been already reproduced almost entire, and then goes on as follows (pp. xi *et seq.*):

Such was the pledge which, on entering upon our editorial labors in 1818, we gave to the public, and such were the views which we then entertained, regarding science and the arts as connected with the interests and honor of our country and of mankind. In the retrospect, we realize a sober but grateful feeling of satisfaction, in having, to the extent of our power, discharged these self-imposed obligations; this feeling is chastened also by a deep sense of gratitude, first to God for life and power continued for so high a purpose; and next, to our noble band of contributors, whose labors are recorded in half a century of volumes, and in more than a quarter of a century of years. We need not conceal our conviction, that the views expressed in these "Introductory Remarks," have been fully sustained by our fellow laborers.

Should we appear to take higher ground than becomes us, we find our vindication in the fact, that we have heralded chiefly the doings and the fame of others. The work has indeed borne throughout "the impress" of editorial unity of design, and much that has flowed from one pen, and not a little from the pens of others, has been without a name. The materials for the pile, have however been selected and brought in, chiefly by other hands, and if the monument which has been reared should prove to be *"aere perennius,"* the honor is not the sole property of the architect; those who have quarried, hewn and polished the granite and the marble, are fully entitled to the enduring record of their names already deeply cut into the massy blocks, which themselves have furnished.

If a retrospective survey of the labors of thirty years on this occasion has rekindled a degree of enthusiasm, it is a natural result of an examination of all our volumes from the contents of which we have endeavored to make out a summary both of the laborers and their works. . . .

The series of volumes must ever form a work of permanent interest on account of its exhibiting the progress of American science during the long period which it covers. Comparing

1817 with 1847, we mark on this subject a very gratifying change. The cultivators of science in the United States were then few—now they are numerous. Societies and associations of various names, for the cultivation of natural history, have been instituted in very many of our cities and towns, and several of them have been active and efficient in making original observations and forming collections.

A summary follows presenting some facts as to the growth of scientific societies and scientific collections in this country during the period involved: Then the striking contrast between 1818 and 1847 in the matter of organized effort toward scientific exploration is discussed, as follows (pp. xvi *et seq.*):

When we began our Journal, not one of the States had been surveyed in relation to its geology and natural history; now those that have not been explored are few in number. State collections and a United States Museum hold forth many allurements to the young naturalist, as well as to the archæologist and the student of his own race. The late Exploring Expedition [Wilkes] with the National Institute, has enriched the capital with treasures rarely equalled in any country, and the Smithsonian Institution recently organized at Washington, is about to begin its labors for the increase and diffusion of knowledge among men.

It must not be forgotten that the American Association of Geologists and Naturalists—composed of individuals assembled from widely separate portions of the Union—by the seven sessions which it has held, and by its rich volume of reports, has produced a concentration and harmony of effort which promise happy results, especially as, like the British Association, it visits different towns and cities in its annual progress.

Astronomy now lifts its exploring tubes from the observatories of many of our institutions. Even the Ohio, which within the memory of the oldest living men, rolled along its dark waters through interminable forests, or received the stains of blood from deadly Indian warfare, now beholds on one of its most beautiful hills, and near its splendid city, a permanent observatory with a noble telescope sweeping the heavens, by the hand of a zealous and gifted observer. At Washington also, under the powerful patronage of the general government, an excellent observatory has been established, and is furnished with superior instruments, under the direction of a vigilant and well instructed astronomer—seconded by able and zealous assistants.

Here also (in Yale College) successful observations have been

made with good instruments, although no permanent building has been erected for an Observatory.

We only give single examples by way of illustration, for the history of the progress of science in the United States, and of institutions for its promotion, during the present generation, would demand a volume. It is enough for our purpose that science is understood and valued, and the right methods of prosecuting it are known, and the time is at hand when its moral and intellectual use will be as obvious as its physical applications. Nor is it to be forgotten that we have awakened an European interest in our researches: general science has been illustrated by treasures of facts drawn from this country, and our discoveries are eagerly sought for and published abroad.

While with our co-workers in many parts of our broad land, we rejoice in this auspicious change, we are far from arrogating it to ourselves. Multiplied labors of many hands have produced the great results. In the place which we have occupied, we have persevered despite of all discouragements, and may, with our numerous coadjutors, claim some share in the honors of the day. We do not say that our work might not have been better done—but we may declare with truth that we have done all in our power, and it is something to have excited many others to effort and to have chronicled their deeds in our annals. Let those that follow us labor with like zeal and perseverance, and the good cause will continue to advance and prosper. It is the cause of truth—science is only embodied and sympathized truth and in the beautiful conception of our noble Agassiz—"it tells the thought of God."

The preface closes with some personal remarks:

In tracing back the associations of many gone-by years, a host of thoughts rush in, and pensive remembrance of the dead who have labored with us casts deep shadows into the vista through which we view the past.

Anticipation of the hour of discharge, when our summons shall arrive, gives sobriety to thought and checks the confidence which health and continued power to act might naturally inspire, were we not reproved, almost every day, by the death of some co-eval, co-worker, companion, friend or patron. This very hour is saddened by such an event,—but we will continue to labor on, and strive to be found at our post of duty, until there is nothing more for us to do; trusting our hopes for a future life in the hands of Him who placed us in the midst of the splendid garniture of this lower world, and who has made not less ample provision for another and a better.

Editorial and financial.—The editorial labors on the Journal were carried by the elder Silliman alone for twenty years from 1818 to 1838. As has been clearly shown in his statements, already quoted, he was, after the first beginning, personally responsible also for the financial side of the enterprise. With volume **34** (1838) the name of Benjamin Silliman, Jr., is added as co-editor on the title page. He was graduated from Yale College the year preceding and at this date was only twenty-one years old. His aid was unquestionably of much service from the beginning and increased rapidly with years and experience. The elder Silliman introduces him in the preface to vol. **34** (1838) and comes back to the subject again in the preface to vol. **50** (1847). The whole editorial situation is here presented as follows:

"During twenty years from the inception of this Journal, the editor labored alone, although overtures for editorial co-operation had been made to him by gentlemen commanding his confidence and esteem, and who would personally have been very acceptable. It was, however, his opinion that the unity of purpose and action so essential to the success of such a work were best secured by individuality; but he made every effort, and not without success, to conciliate the good will and to secure the assistance of gentlemen eminent in particular departments of knowledge. On the title page of No. 1, vol. **34**, published in July, 1838, a new name is introduced: the individual to whom it belongs having been for several years more or less concerned in the management of the Journal, and from his education, position, pursuits and taste, as well as from affinity, being almost identified with the editor, he seemed to be quite a natural ally, and his adoption into the editorship was scarcely a violation of individual unity. His assistance has proved to be very important:—his near relation to the senior editor prevents him from saying more, while justice does not permit him to say less."

As is distinctly intimated in the preceding paragraph the elder Silliman was fortunate in obtaining the assistance in his editorial labors of numerous gentlemen interested in the enterprise. Their coöperation provided many of the scientific notices, book reviews and the like contained in the Miscellany with which each number closed. It is impossible, at this date, to render the credit due to Silliman's helpers or even to mention them by

name. Very early Asa Gray was one of these as occasional notes are signed by his initials. Dr. Levi Ives of New Haven was another. Prof. J. Griscom of Paris also sent numerous contributions even as early as 1825 (see **9**, 154, 1825; **22**, 192, 1832; **24**, 342, 1833, and others).

Some statements have already been quoted from the early volumes as to the business part of Silliman's enterprise. The subject is taken up more fully in the preface to volume **50** (1847). No one can fail to marvel at the energy and optimism required to push the Journal forward when conditions must have been so difficult and encouragement so scanty. He says (pp. iii, iv):

This Journal first appeared in July, 1818, and in June, 1819, the first volume of four numbers and 448 pages was completed. This scale of publication, originally deemed sufficient, was found inadequate to receive all the communications, and as the receipts proved insufficient to sustain the expenses, the work, having but three hundred and fifty subscribers, was, at the end of the year, abandoned by the publishers.

An unprofitable enterprise not being attractive to the trade, ten months elapsed before another arrangement could be carried into effect, and, therefore, No. 1 of vol. **2** was not published until April, 1820. The new arrangement was one of mutual responsibility for the expenses, but the Editor was constrained nevertheless to pledge his own personal credit to obtain from a bank the funds necessary to begin again, and from this responsibility he was, for a series of years, seldom released. The single volume per annum being found insufficient for the communications, two volumes a year were afterward published, commencing with the second volume.

The publishers whose names appear on the title page of the four numbers of the first volume are "J. Eastburn & Co., Literary Rooms, Broadway, New York" and Howe & Spalding, New Haven." For the second volume and those immediately following the corresponding statement "printed and published by S. Converse [New Haven] for the Editor."

Silliman adds (p. iv):

At the conclusion of vol. **10**, in February, 1826, the work was again left upon the hands of its Editor; all its receipts had been absorbed by the expenses, and it became necessary now to pay a heavy sum to the retiring publisher, as an equivalent for his

copies of previous volumes, as it was deemed necessary either to control the work entirely or to abandon it. The Editor was not willing to think of the latter, especially as he was encouraged by public approbation, and was cheered onward in his labors by eminent men both at home and abroad, and he saw distinctly that the Journal was rendering service not only to science and the arts, but to the reputation of his country. He reflected, moreover, that in almost every valuable enterprise perseverance in effort is necessary to success. He being now sole proprietor, a new arrangement was made for a single year, the publishers being at liberty, at the end of that time, to retire, and the Editor to resume the Journal should he prefer that course.

The latter alternative he adopted, taking upon himself the entire concern, including both the business and the editorial duties, and of course, all the correspondence and accounts. From that time the work has proceeded without interruption, two volumes per annum having been published for the last twenty years; and its pecuniary claims ceased to be onerous, although its means have never been large. . . .

Later in the same preface he adds (p. xiv):

It may be interesting to our readers to know something of the patronage of the Journal. It has never reached one thousand paying subscribers, and has rarely exceeded seven or eight hundred—for many years it fluctuated between six and seven hundred.

It has been far from paying a reasonable editorial compensation; often it has paid nothing, and at present it does little more than pay its bills. The number of engravings and the extra labor in printer's composition, cause it to be an expensive work, while its patronage is limited.

It is difficult at this date to give any adequate statement of the amount of encouragement and active assistance given to Silliman by his scientific colleagues in New Haven and elsewhere—a subject earlier alluded to. It is fortunately possible, however, to acknowledge the generous aid received by the Journal in the early days from a source near at hand. It has already been noted in another place that the dawning activity of science at New Haven was recognized by the founding of the "Connecticut Academy of Arts and Sciences," formally established at New Haven in 1799 and the third scientific body to be organized in this country. From the beginning of

the Journal in 1818, the Connecticut Academy freely gave its support both in papers for publication and at least on one occasion later it gave important financial aid. Upon the occasion of the celebration of the centennial anniversary of the Academy on October 11, 1899, Professor, later Governor, Baldwin, the president of the Academy, discusses this subject in some detail. He says in part:

> To support his [Silliman's] undertaking, a vote had been passed in February [1818], "that the Committee of Publication may allow such of the Academy's papers as they think proper, to be published in Mr. Silliman's Scientific Journal."
>
> Free use was made of this authority, and a large part of the contents of the Journal was for many years drawn from this source. In some cases this fact was noted in publication;[2] but in most it was not. . . .
>
> In 1826, when the Journal was in great need of financial support, the Academy further voted to pay for a year the cost of printing such of its papers as might be published in it. In Baldwin's Annals of Yale College, published in 1831, it is described as a publication "honorable to the science of our common country," and having "an additional value as being adopted as the acknowledged organ of the Connecticut Academy of Arts and Sciences."

Many active campaigns were carried on over the country through paid agents to obtain new subscribers for the Journal and it was doubtless due to these efforts that the nominal subscription list was, at times, as already noted, relatively large as compared with that of a later date. The new subscribers in many cases, however, did not remain permanently interested, often failed to pay their bills, and the uncertain and varying demand upon the supply of printed copies was doubtless one reason why many single numbers became early out of print.

An interesting sidelight is thrown upon the efforts of Silliman to interest the public in his work, at its beginning, by a letter to the editor from Thomas Jefferson, then seventy-five years of age. The writer is indebted to Mr. Robert B. Adam of Buffalo for a copy of this letter and its interest justifies its being reproduced here entire. The letter is as follows:

Monticello, Apr. 11. '18.

Sir

The unlucky displacement of your letter of Mar 3 has been the cause of delay in my answer. altho' I have very generally withdrawn from subscribing to or reading periodical publications from the love of rest which age produces, yet I willingly subscribe to the journal you propose from a confidence that the talent with which it will be edited will entitle it to attention among the things of select reading for which alone I have time now left. be so good as to send it by mail, and the receipt of the 1st number will be considered as announcing that the work is commenced and the subscription money for a year shall be forwarded. Accept the assurance of my great esteem and respect.

Th. Jefferson

Professor Silliman.

Contributors.—An interesting summary is also given by Silliman of the contributors to the Journal and the extent of their work (vol. 50, pp. xii, xiii); he says:

We find that there have been about 600 contributors of original matter to the Journal, and we have the unexpected satisfaction of believing that probably five-sixths of them are still living; for we are not certain that more than fifty are among the dead; of perhaps fifty more we are without information, and if that additional number is to be enrolled among the "stelligeri," we have still 500 remaining. Among them are not a few of the veterans with whom we began our career, and several of these are still active contributors. Shall we then conclude that the peaceful pursuits of knowledge are favorable to long life? This we think is, *cæteris paribus,* certainly true: but in the present instance, another reason can be assigned for the large amount of survivorship. As the Journal has advanced and death has removed its scientific contributors, younger men and men still younger, have recruited the ranks, and volunteers have enlisted in numbers constantly increasing, so that the flower of the host are now in the morning and meridian of life.

We have been constantly advancing, like a traveller from the equinoctial towards the colder zones,—as we have increased our latitude, stars have set and new stars have risen, while a few planetary orbs visible in every zone, have continued to cheer us on our course.

The number of articles, almost exclusively original, contained in the Journal is about 1800, and the Index will show how many

have been contributed by each individual; we have doubtless included in this number *some few* articles republished from foreign Journals—but we think they are even more than counterbalanced by original communications without a name and by editorial articles, both of which have been generally omitted in the enumeration.

Of smaller articles and notices in the Miscellany, we have not made any enumeration, but they evidently are more numerous than the regular articles, and we presume that they may amount to at least 2500.

Of party, either in politics or religion, there is no trace in our work; of personalities there are none, except those that relate to priority of claims or other rights of individuals. Of these vindications the number is not great, and we could heartily have wished that there had been no occasion for any.

General Scope of Articles.—Many references will be found in the chapters following which throw light upon the character and scope of the papers published in the Journal, particularly in its early years; a few additional statements here may, however, prove of interest.

One feature that is especially noticeable is the frequent publication of articles planned to place before the readers of the Journal in full detail subjects to which they might not otherwise have access. These are sometimes translations; sometimes republications of articles that had already appeared in English periodicals; again, they are exhaustive and critical reviews of important memoirs or books. The value of this feature in the early history of the Journal, when the distribution of scientific literature had nothing of the thoroughness characteristic of recent years, is sufficiently obvious.

It is also interesting to note the long articles of geological description and others giving lists of mineral or botanical localities. Noteworthy, too, is the attempt to keep abreast of occurring phenomena as in the many notes on tornadoes and storms by Redfield, Loomis, etc.; on auroras at different localities; on shooting stars by Herrick, Olmstead and others.

The wide range of topics treated of is quite in accordance with the plan of the editor as given on an earlier page. Some notes, taken more or less at random, may serve to illustrate this point. An extended and quite

technical discussion of "Musical Temperament" opens the first number (**1**, pp. 9-35) and is concluded in the same volume (pp. 176-199). An article on "Mystery" is given by Mark Hopkins, A.M., "late a tutor of Williams College" (**13**, 217, 1828). There is an essay on "Gypsies" by J. Griscom (from the Revue Encyclopédique) in volume **24** (pp. 342-345, 1833), while some notes on American gypsies are added in vol. **26** (p. 189, 1834). The "divining rod" is described at length in vol. **11** (pp. 201-212, 1826), but without giving any comfort to the credulous; on the contrary the last paragraph states that "the pretensions of diviners are worthless, etc." A long article by J. Finch on the forts of Boston harbour appeared in 1824 (**8**, 338-348); the concluding paragraph seems worthy of quotation:

> "Many centuries hence, if despotism without, or anarchy within, should cause the republican institutions of America to fade, then these fortresses ought to be destroyed, because they would be a constant reproach to the people; but until that period, they should be preserved as the noblest monuments of liberty."

The promise to include the fine arts is kept by the publication of various papers, as of the Trumbull paintings (**16**, 163, 1829); also by a series of articles on "architecture in the United States" (**17**, 99, 1830; **18**, 218, 220, 1830) and others. Quite in another line is the paper by J. W. Gibbs (**33**, 324, 1838) on "Arabic words in English." A number of related linguistic papers by the same author are to be found in other volumes. Papers in pure mathematics are also not infrequent, though now not considered as falling within the field of the Journal.

Applied science takes a prominent place through all the volume of the First Series. An interesting paper is that on Eli Whitney, containing an account of the cotton gin; this is accompanied by an excellent portrait (**21**, 201-264, 1832). The steam engine and its application are repeatedly discussed and in the early volumes brief accounts are given of the early steamboats in use; for example, between Stockholm and St. Petersburg (**2**, 347, 1820); Trieste and Venice (**4**, 377, 1822); on the Swiss Lakes

(**6**, 385, 1823). The voyage of the first Atlantic steamboat, the "Savannah," which crossed from Savannah to Liverpool in 1819, is described (**38**, 155, 1840); mention is also made of the "first iron boat" (**3**, 371, 1821; **5**, 396, 1822). A number of interesting letters on "Steam Navigation" are given in vol. **35**, 160, 162, 332, 333, 336; some of the suggestions seem very quaint, viewed in the light of the experience of to-day.

A very early form of explosive engine is described at length by Samuel Morey (**11**, 104, 1826); this is an article that deserves mention in these days of gasolene motors. Even more interesting is the description by Charles Griswold (**2**, 94, 1820) of the first *submarine* invented by David Bushnell and used in the Revolutionary War in August, 1776. An account is also given of a dirigible balloon that may be fairly regarded as the original ancestor of the Zeppelin (see **11**, 346, 1826). The whole subject of aërial navigation is treated at length by H. Strait (**25**, pp. 25, 26, 1834) and the expression of his hopes for the future deserve quotation:

"Conveyance by air can be easily rendered as safe as by water or land, and more cheap and speedy, while the universal and uniform diffusion of the air over every portion of the earth, will render aërial navigation preferable to any other. To carry it into effect, there needs only an immediate appeal on a sufficiently large scale, to experiment; reason has done her part, when experiment does hers, nature will not refuse to sanction the whole. Aërial navigation will present the works of nature in all their charms; to commerce and the diffusion of knowledge, it will bring the most efficient aid, and it can thus be rendered serviceable to the whole human family."

A subject of quite another character is the first discussion of the properties of chloroform (chloric ether) and its use as an anæsthetic (Guthrie, **21**, 64, 405, 1832; **22**, 105, 1832; Levi Ives, **21**, 406). Further interesting communications are given of the first analyses of the gastric juice and the part played by it in the process of digestion. Dr. William Beaumont of St. Louis took advantage of a patient who through a gun-shot wound was left with a permanent opening into his stomach through which the gastric juice could be drawn off. The

results of Dr. Beaumont and of Professor Robley Dunglison, to whom samples were submitted, are given in full in the life of Beaumont by Jesse S. Myer (St. Louis, 1912). The interest of the matter, so far as the Journal is concerned, is chiefly because Dr. Beaumont selected Professor Silliman as a chemist to whom samples for examination were also submitted. An account of Silliman's results is given in the Beaumont volume referred to (see also **26**, 193, 1834). Desiring the support of a chemist of wider experience in organic analysis, he also sent a sample through the Swedish consul to Berzelius in Stockholm. After some months the sample was received and it is interesting to note in a perfectly fresh condition; it is to be regretted, however, that the Swedish chemist failed to add anything to the results already obtained in this country (**27**, 40b, 1835).

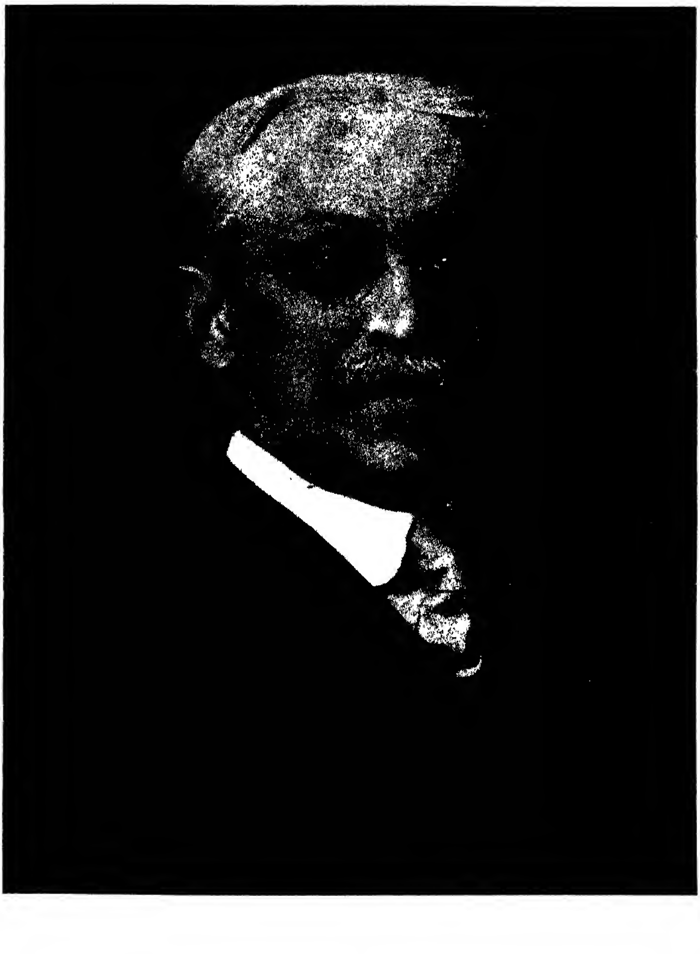
The above list, which might be greatly extended, seems to leave little ground for the implied criticism replied to by Silliman as follows (**16**, p. v, 1829):

> A celebrated scholar, while himself an editor, advised me, in a letter, to introduce into this Journal as much *"readable"* matter as possible: and there was, pretty early, an earnest but respectful recommendation in a Philadelphia paper, that Literature, in imitation of the London Quarterly Journal of Science, &c. should be in form, inscribed among the titles of the work.

The Second, Third and Fourth Series.

The Second Series of the Journal, as already stated, began with January, 1846. Up to this time the publication had been a quarterly or two volumes annually of two numbers each. From 1846 until the completion of an additional fifty volumes in 1871, the Journal was made a bimonthly, each of the two yearly volumes having three numbers each. Furthermore, a general index was given for each period of five years, that is for every ten volumes.

Much more important than this change was the addition to the editorial staff of James Dwight Dana, Silliman's son-in-law. Dana returned from the four-years cruise of the Wilkes Exploring Expedition in 1842; he settled in New Haven, was married in 1844, and in 1850



Edward S. Dana

was appointed Silliman professor of Geology in Yale College. He was at this time actively engaged in writing his three quarto reports for the Expedition and hence did not begin his active professional duties in Yale College until 1856. Part of his inaugural address was quoted on an earlier page.

Dana had already performed the severe labor of preparing the complete index to the First Series, a volume of about 350 pages finally issued in 1847. From the beginning of the Second Series he was closely associated with his brother-in-law, the younger Silliman. Later the editorial labor devolved more and more upon him and the larger part of this he carried until about 1890. His work was, however, somewhat interrupted during periods of ill health. This was conspicuously true during a year's absence in Europe in 1859-60, made necessary in the search for health; during these periods the editorial responsibility rested entirely upon the younger Silliman. Of Dana's contributions to science in general this is not the place to speak, nor is the present writer the one to dwell in detail upon his work for the Journal. This subject is to such an extent involved in the history of geology and zoology, the subjects of several succeeding chapters, that it is adequately presented in them.

It may, however, be worth stating that in the bibliography accompanying the obituary notice of Dana (**49**, 329-356, 1895) some 250 titles of articles in the Journal are enumerated; these aggregate approximately 2800 pages. The number of critical notes, abstracts, book reviews, etc., could be also given, were it worth while, but what is much more significant in this connection, than their number or aggregate length, is the fact that these notices are in a large number of cases—like those of Gray in botany—minutely critical and original in matter. They thus give the writer's own opinion on a multitude of different subjects. It was a great benefit to Dana, as it was to science also, that he had this prompt means at hand of putting before the public the results of his active brain, which continued to work unceasingly even in times of health prostration.

This may be the most convenient place to add that as Dana became gradually less able to carry the burden of

the details involved in editing the Journal in addition to his more important scientific labors, particularly from 1890 on, this work devolved more and more upon his son, the present editor, whose name was added to the editorial staff in 1875, with volume 9, of the Third Series. The latter has served continuously until the present time, with the exception of absences, due to ill health, in 1893-94 and in 1903; during the first of these Professor Henry S. Williams and during the second Professor H. E. Gregory occupied the editorial chair.

The Third Series began in 1871, after the completion of the one-hundredth volume from the beginning in 1818. At this date the Journal was made a monthly and as such it remains to-day. Fifty volumes again completed this series, which closed in 1895.

The Fourth Series began with January, 1896, and the present number for July, 1918, is the opening one of the forty-sixth volume or, in other words,—the one hundred and ninety-sixth volume of the entire issue since 1818. The Fourth Series, according to the precedent established, will end with 1920.

Associate Editors.—In 1851 the new policy was introduced of adding "Associate Editors" to the staff. The first of these was Dr. Wolcott Gibbs of Cambridge. He began his duties with the eleventh volume of the Second Series in 1851 and continued them with unceasing care and thoroughness for more than twenty years. In a note dated Jan. 1, 1851 (**11**, 105), he says:

> It is my intention in future to prepare for the columns of this Journal abstracts of the more important physical and chemical memoirs contained in foreign scientific journals, accompanied by references, and by such critical observations as the occasion may demand. Contributions of a similar character from others will of course not be excluded by this arrangement, but I shall hold myself responsible only for those notices which appear over my initials.

The departments covered by Dr. Gibbs, in his excellent monthly contributions, embraced chemistry and physics, and these subjects were carried together until 1873 when they were separated and the physical notes were fur-

nished, first by Alfred M. Mayer and later successively by E. C. Pickering (from 1874), J. P. Cooke (from 1877), and John Trowbridge (from 1880). The first instalment of the long series of notes in chemistry and chemical physics by George F. Barker was printed in volume **50**, 1870. He came in at first to occasionally relieve Dr. Gibbs, but soon took the entire responsibility. His name was placed among the associate editors on the cover in 1877 and two years later Dr. Gibbs formally retired. It may be added that from the beginning in 1851 to the present time, the notes in "Chemistry and Physics" have been continued almost without interruption.

The other departments of science have been also fully represented in the notes, abstracts of papers published, book notices, etc., of the successive numbers, but as with the chemistry and physics the subject of botany was long treated in a similar formal manner. For the notes in this department, the Journal was for many years indebted to Dr. Asa Gray, who became associate editor in 1853, two years after Gibbs, although he had been a not infrequent contributor for many years previously. Gray's contributions were furnished with great regularity and were always critical and original in matter. They formed indeed one of the most valuable features of the Journal for many years; as botanists well appreciate, and, as Professor Goodale has emphasized in his chapter on botany, Gray's notes are of vital importance in the history of the development of his subject. With Gray's retirement from active duty, his colleague, George W. Goodale, took up the work in 1888 and in 1895 William G. Farlow, also of Cambridge, was added as an associate editor in cryptogamic botany. At this time, however, and indeed earlier, the sphere of the Journal had unavoidably contracted and botany perforce ceased to occupy the prominent place it had long done in the Journal pages.

This is not the place to present an appreciation of the truly magnificent work of Asa Gray. It may not be out of place, however, to call attention to the notice of Gray written for the Journal by his life-long friend, James D. Dana (**35**, 181, 1888). The opening paragraph is as follows:

"Our friend and associate, Asa Gray, the eminent botanist of America, the broad-minded student of nature, ended his life of unceasing and fruitful work on the 30th of January last. For thirty-five years he has been one of the editors of this Journal, and for more than fifty years one of its contributors; and through all his communications there is seen the profound and always delighted student, the accomplished writer, the just and genial critic, and as Darwin has well said, 'The lovable man.'"

The third associate editor, following Gray, was Louis Agassiz, whose work for science, particularly in his adopted home in this country, calls for no praise here. His term of service extended from 1853 to 1866 and, particularly in the earlier years, his contributions were numerous and important. The next gentleman in the list was Waldo I. Burnett, of Boston, who served one year only, and then followed four of Dana's colleagues in New Haven, of whose generosity and able assistance it would be impossible to say too much. These gentlemen were Brush in mineralogy; Johnson in chemistry, particularly on the agricultural side; Newton in mathematics and astronomy, whose contributions will be spoken of elsewhere; and Verrill—a student of Agassiz—in zoology.

All of these gentlemen. besides their frequent and important original articles, were ever ready not only to give needed advice, but also, to furnish brief communications, abstracts of papers and book reviews, and otherwise to aid in the work. Verrill particularly furnished the Journal a long list of original and important papers, chiefly in systematic zoology, extending from 1865 almost down to the present year. His abstracts and book notices also were numerous and trenchant and it is not too much to say that without him the Journal never could have filled the place in zoology which it so long held. Much later the list of New Haven men was increased by the addition of Henry S. Williams (1894), and O. C. Marsh (1895).

Of the valuable work of those more or less closely associated in the conduct of the Journal at the present time, it would not be appropriate to speak in detail. It must suffice to say that the services rendered freely by them have been invaluable, and to their aid is due a large part of the success of the Journal, especially since the Fourth

Wolcott Gibbs

Series began in 1896. But even this statement is inadequate, for the editor-in-chief has had the generous assistance of other gentlemen, whose names have not been placed on the title page, and who have also played an important part in the conduct of the Journal. This policy, indeed, is not a matter of recent date. Very early in the First Series, Professor Griscom of Paris, as already noted, furnished notes of interesting scientific discoveries abroad. Other gentlemen have from time to time acted in the same capacity. The most prominent of them was Professor Jerome Nicklès of Nancy, France, who regularly furnished a series of valuable notes on varied subjects, chiefly from foreign sources, extending from 1852 to 1869. On the latter date he met an untimely death in his laboratory in connection with experiments upon hydrofluoric acid (**47**, 434, 1869).

It may be added, further, that one of the striking features about the Journal, especially in the earlier half century of its existence, is the personal nature of many of its contributions, which were very frequently in the form of letters written to Benjamin Silliman or J. D. Dana. This is perhaps but another reflection of the extent to which the growth of the magazine centered around these two men, whose wide acquaintance and broad scientific repute made of the Journal a natural place to record the new and interesting things that were being discovered in science.

The following list gives the names and dates of service, as recorded on the Journal title pages, of the gentlemen formally made Associate Editors:

Wolcott Gibbs	(2) 11, 1851	to	(3)	18, 1879
Asa Gray	" 15, 1853	"	"	34, 1887
Louis Agassiz	" 16, 1853	"	(2)	41, 1866
Waldo I. Burnett	" 16, 1853	"	"	17, 1853
George J. Brush	" 35, 1863	"	(3)	18, 1879
Samuel W. Johnson	" 35, 1863	"	"	18, 1879
Hubert A. Newton	(2) 38, 1864	to	(4)	1, 1896
Addison E. Verrill	" 47, 1869			
Alfred M. Mayer	(3) 5, 1873	to	(3)	6, 1873
Edward C. Pickering	" 7, 1874	"	"	13, 1877
George F. Barker	" 14, 1877	"	(4)	29, 1910
Josiah P. Cooke	" 14, 1877	"	(3)	47, 1894

John Trowbridge	(3) 19, 1880		
George W. Goodale	" 35, 1888		
Henry S. Williams	47, 1894		
Henry P. Bowditch	49, 1895	to (4)	8, 1899
William G. Farlow	49, 1895		
Othniel C. Marsh	49, 1895	to (4)	6, 1899
Henry A. Rowland	(4) 1, 1896	" "	10, 1900
Joseph S. Diller	" 1, 1896		
Louis V. Pirsson ...	7, 1899		
William M. Davis ..	9, 1900		
Joseph S. Ames	12, 1901		
Horace L. Wells	18, 1904		
Herbert E. Gregory	18, 1904		
Horace S. Uhler	33, 1912		

Present and Future Conditions.

The field to be occupied by the "American Journal of Science and Arts," as seen by its founder in 1818 and presented by him in the first number, as quoted entire on an earlier page, was as broad as the entire sphere of science itself. It thus included all the departments of both pure and applied science and extended even to music and fine arts also. As the years went by, however, and the practical applications of science greatly increased, technical journals started up, and the necessity of cultivating this constantly expanding field diminished. It was not, however, until January, 1880, that "the Arts" ceased to be a part of the name by which the Journal was known.

About the same date also—or better a little earlier—began an increasing development of scientific research, particularly as fostered by the graduate schools of our prominent universities. The full presentation of this subject would require much space and is indeed unnecessary as the main facts must be distinct in the mind of the reader. It is only right, however, that the large part played in this movement by the Johns Hopkins University (founded in 1876) should be mentioned here.

As a result of this movement, which has been of great benefit in stimulating the growth of science in the country, many new journals of specialized character have come into existence from time to time. Further localization and specialization of scientific publication have

resulted from the increased activity of scientific societies and academies at numerous centers and the springing into existence thereby of new organs of publication through them, as also through certain of the Government Departments, the Carnegie Institution, and certain universities and museums.

As bearing upon this subject, the following list of the more prominent scientific periodicals started in this country since 1867 is not without interest:

1867– .	American Naturalist.
1875– .	Botanical Bulletin; later Botanical Gazette.
1879–1913.	American Chemical Journal.
1880–1915.	School of Mines Quarterly.
1883– .	Science.
1885– .	Journal of Heredity.
1887– .	Journal of Morphology.
1887–1908.	Technology Quarterly.
1888–1905.	American Geologist.
1891– .	Journal of Comparative Neurology.
1893– .	Journal of Geology.
1893– .	Physical Review.
1895– .	Astrophysical Journal.
1896– .	Journal of Physical Chemistry.
1896– .	Terrestrial Magnetism.
1897–1899.	Zoological Bulletin; followed by
1900– .	Biological Bulletin.
1901– .	American Journal of Anatomy.
1904– .	Journal of Experimental Zoology.
1905– .	Economic Geology.
1906– .	Anatomical Record.
1907– .	Journal of Economic Entomology.
1911– .	Journal of Animal Behavior.
1914– .	American Journal of Botany.
1916– .	Genetics.
1918– .	American Journal of Physical Anthropology

The result of the whole movement has been of necessity to narrow, little by little, the sphere of a general scientific periodical such as the Journal has been from the beginning. The exact change might be studied in detail by tabulating as to subjects the contents of successive volumes, decade by decade, from 1870 down. It is sufficient, here, however, to recognize the general fact that while the number of original papers published in the

periodicals of this country, in 1910, for example, was very many times what it was in 1825, a large part of these have naturally found their home in periodicals devoted to the special subject dealt with in each case. That this movement will continue, though in lessened degree now that the immediate demand is measurably satisfied, is to be expected. At the same time it has not seemed wise, at any time in the past, to formally restrict the pages of the Journal to any single group of subjects. The future is before us and its problems will be met as they arise. At the moment, however, there seems to be still a place for a scientific monthly sufficiently broad to include original papers of important general bearing even if special in immediate subject. In this way it would seem that "Silliman's Journal" can best continue to meet the ideals of its honored founder, modified as they must be to meet the change of conditions which a century of scientific investigation and growth have wrought. Incidentally it is not out of place to add that a self-supporting, non-subsidized scientific periodical may hope to find a larger number of subscribers from among the workers in science and the libraries if it is not too restricted in scope.

The last subject touched upon introduces the essential matter of financial support without which no monthly publication can survive. With respect to the periodicals of recent birth, listed above, it is safe to say that some form of substantial support or subsidy—often very generous—is the rule, perhaps the universal one. This has never been the case with the American Journal. The liberality and broad-minded attitude of Yale College in the early days, and of the Yale University that has developed from it, have never been questioned. At the same time the special conditions have been such as to make it desirable that the responsibility of meeting the financial requirements should be carried by the editors-in-chief. At present the Yale Library gives adequate payment for certain publications received by the Journal in exchange, though for many years they were given to it as a matter of course, free of charge. Beyond this there is nothing approaching a subsidy.

The difficulties on the financial side met with by the elder Silliman have been suggested, although not adequately

presented, in the various statements quoted from early volumes. The same problems in varying degree have continued for the past sixty years. Since 1914 they have been seriously aggravated for reasons that need not be enlarged upon. Prior to that date the subscription list had, for reasons chiefly involved in the development of special journals, been much smaller than the number estimated by Silliman, for example, in volume **50** (p. xiv), although there has been this partial compensation that the considerable number of well-established libraries on the subscription list has meant a greater degree of stability and a smaller proportion of bad accounts. The past four years, however, the Journal, with all similar undertakings here and elsewhere, has been compelled to bear its share of the burden of the world war in diminished receipts and greatly increased expenses. It is gratifying to be able to acknowledge here the generosity of the authors, or of the laboratories with which they have been connected, in their willingness not infrequently to give assistance, for example, in the payment of more or less of the cost of engravings, or in a few special cases a large portion of the total cost of publication. In this way the problem of ways and means, constantly before the editor who bears the sole responsibility, has been simplified.

It should also be stated that as those immediately interested have looked forward to the present anniversary, it has been with the hope that this occasion might be an appropriate one for the establishment of a "Silliman Fund" to commemorate the life and work of Benjamin Silliman. The income of such a fund would lift from the University the burden that must unavoidably fall upon it when the responsibility for the conduct of the Journal can no longer be carried by members of the family including the editor and—as in years long past—a silent partner whose aid on the business side has been essential to the efficiency and economy of the enterprise. Present conditions are not favorable for such a movement, although something has been already accomplished in the desired direction. At the present time every patriotic citizen must feel it his first duty to give his savings as well as his spare income to the support of the

National Government in the world struggle for freedom in which it is taking part. But, whatever the exact condition of the future may be, it cannot be questioned that the Journal founded by Benjamin Silliman in 1818 will survive and will continue to play a vital part in the support and further development of science.

The present year of 1918 finds the world at large, and with it the world of science, painfully crushed beneath the overwhelming weight of a world war of unprecedented severity. The four terrible years now nearly finished have seen a fearful destruction of life and property which must have a sad influence on the progress of science for many years to come. Only in certain restricted lines has there been a partial compensation in the stimulating influence due to the immediate necessities connected with the great conflict. One hundred years ago "the reign of war" was keenly in the mind of the editor in beginning his work, but for him, happily, the long period of the Napoleonic wars was already in the past, as also the brief conflict of 1812, in which this country was engaged and in which Silliman himself played a minor part. We, too, must believe, no matter how serious the outlook of the present moment, that a fundamental change will come in the not distant future; the nations of the world must sooner or later turn once more to peaceful pursuits and the scientific men of different races must become again not enemies but brothers engaged in the common cause of uplifting human life. The peace that we look forward to to-day is not for this country alone, but a peace which shall be a permanent blessing to the entire world for ages to come.

Note.—The portrait on the next leaf has been reproduced from the plate in volume **50** (1847) of the Journal. The original painting was made by H. Willard in 1835, when Silliman was in Boston engaged in delivering the Lowell lectures; he was then nearly fifty-six years of age. The engraving, as he states elsewhere, was made from this painting for the Yale Literary Magazine, and was published in the number for December, 1839.

B. Silliman

It is interesting to quote the remarks with which the editor introduces the portrait (50, xviii, 1847). He says:

The portrait prefixed to this volume was engraved for a very different purpose and for others than the patrons of this Journal. It has been suggested by friends, whose judgment we are accustomed to respect, that it ought to find a place here, since it is regarded as an authentic, although, perhaps, a rather austere resemblance. In yielding to this suggestion, it may be sufficient to quote the sentiment of Cowper on a similar occasion, who remarked—"that after a man has, for many years, turned his mind *inside out* before the world, it is only affectation to attempt to hide his face."

Notes.

[1] The statements given are necessarily much condensed, without an attempt to follow all changes of title; furthermore, the dates of actual publication for the academies given above are often somewhat vaguely recorded. For fuller information see Scudder's "Catalogue of Scientific Serials, 1633-1876," Cambridge, 1876; also H. Carrington Bolton's "Catalogue of Scientific and Technical Periodicals, 1665-1882" (Smithsonian Institution, 1885). The writer is much indebted to Mr. C. J. Barr, Assistant Librarian of Yale University Library, for his valuable assistance in this connection

[2] The following footnote accompanies the opening article of the first volume of the Journal. "From the MS. papers of the Connecticut Academy, now published by permission." Similar notes appear elsewhere. Ed.

II

A CENTURY OF GEOLOGY.—THE PROGRESS OF HISTORICAL GEOLOGY IN NORTH AMERICA

By CHARLES SCHUCHERT

Introduction.

THE American Journal of Science, "one of the greatest influences in American geology," founded in 1818, has published a little more than 92,000 pages of scientific matter. Of geology, including mineralogy, there appear to be upward of 20,000 pages. What a vast treasure house of geologic knowledge is stored in these 194 volumes, and how well the editors have lived up to their proposed "plan of work" as stated in the opening volume, where Silliman says: "It is designed as a deposit for original American communications" in "the physical sciences . . . and especially our mineralogy and geology" (1, v, 1818)! Not only is it the oldest continuously published scientific journal of this country, but it has proved itself to be "perhaps the most important geological periodical in America" (Merrill). It is impossible to adequately present in this memorial volume of the Journal the contents of the articles on the geological sciences.

Editor Silliman was not only the founder of the Journal, but the generating center for the making of geologists and promoting geology during the rise of this science in America. For nearly three decades, the workers came to him for counsel and help, and he had a kind paternal word for them all. This influence is also shown in the many letters which were addressed to him, and which he published in the Journal. A similar influence, paternal care, and constructive criticism were continued

by James D. Dana, and especially in his earlier career as editor.

Not including mineralogy, there are in the Journal upward of 1500 distinct articles on geology. Of these, over 400 are on vertebrate paleontology, about 325 on invertebrate paleontology, and 90 on paleobotany. Of articles bearing on historical geology there are about 160, and on stratigraphic geology more than 360. In addition to all this, there are more than 2000 pages of geologic matter relating to books and of letters communicated to the editors Silliman and Dana. We may summarize with Doctor Merrill's statement in his well-known Contributions to the History of American Geology:

"From its earliest inception geological notes and papers occupied a prominent place in its pages, and a perusal of the numbers from the date of issue down to the present time will, alone, afford a fair idea of the gradual progress of American geology."

Before presenting a synopsis of the more important steps in the progress of historical geology in America, it will be well to introduce a rapid survey of the rise of geology in Europe, for, after all, American geology grew out of that of England, France and Germany. This dependence was conspicuously true during the first four decades of the previous century. With the rise of the first New York State Survey (1836-1843) and that of Pennsylvania (1836-1844, 1858), American geology became more or less independent of Europe. Finally, this article will conclude with a survey of the rise of paleometeorology, paleogeography, evolution, and invertebrate paleontology.

The Rise of Geology in Europe.

Mineral Geology.—The geological sciences had their rise in the study of minerals as carried on by the German chemist and physician George Bauer (1494-1555), better known as Agricola. Bauer originated the critical study of minerals, but did not distinguish his "fossilia," the remains of organisms, from the inorganic crystal forms. Mineral geology endured until the close of the eighteenth century.

Cosmogonists.—*Then came the expounders of the* earth's origin, the cosmogonists of the sixteenth to the end of the eighteenth centuries. The fashion of this time was to write histories of the earth derived out of the imagination.

Earliest Historical Geology.—Even though Giovanni Arduino (1713-1795) of Padua was not the first to classify the rocks into three series according to their age, he did this more clearly than any one else before his time. The rocks about Verona he grouped in 1759 into Primary, Secondary, Tertiary, and Volcanic. This three-fold classification came into general use, though modified with time.

Early in the nineteenth century it had become plain that formations of very varying ages were included in each one of the three series. Through the study of the fossils and the recognition of the fact that mountain ranges have been raised at various times, causing younger fossiliferous strata to take on the characters of the Primary, it was seen that these terms of Arduino had lost their original significance.

The first one to describe in detail a local stratigraphic sequence was Johann Gottlob Lehmann (died 1767). In 1756 he published "one of the classics of geological literature," distinguishing clearly thirty successive sedimentary deposits, some of which he said had fossils, but he did not use them to distinguish the strata.

What Lehmann did for the Permian system, George Christian Füchsel (1722-1773) did even better for the Triassic of Thuringia, in 1762 and 1773. He pointed out not only the sequence, but also how the gently inclined strata rest upon the older upturned masses of the mountains; also that some formations have only marine fossils, while others have only terrestrial forms and thus indicate the proximity of land. The deformed strata he thought had fallen into the hollows within the earth, great caverns that had also consumed much of the oceanic waters and had in so doing greatly lowered the sea-level. It was Füchsel who first introduced the theory of universal formations, and who defined the term formation, using it as we now do, system or period. Even though Lehmann and Füchsel showed that there

was a definite order and process in the formation of the earth's crust, their example was barren of followers until the beginning of the eighteenth century.

Wernerian Geology or Geognosy.—We come now to the time of Abraham Gottlob Werner (1749-1817), who from 1775 to 1817 was professor of mining and mineralogy in the Freiberg Academy of Mines. Geikie, in his most interesting Founders of Geology, says that Werner "bulks far more largely in the history of geology than any of those with whom up to the present we have been concerned—a man who wielded an enormous authority over the mineralogy and geology of his day." "Although he did great service by the precision of his lithological characters and by his insistence on the doctrine of geological succession, yet as regards geological theory, whether directly by his own teaching, or indirectly by the labors of his pupils and followers, much of his influence was disastrous to the higher interests of geology."

Werner arranged the crust of the earth into a series of formations, as had been done previously by Lehmann and Füchsel, and one of his fundamental postulates was that all rocks were chemically precipitated in the ocean as "universal formations." For this reason Werner's school were called the Neptunists. Nowhere, however, did he explain how and where the deep and primitive ocean had disappeared.

According to Werner, the first formed or oldest rocks were the chemically deposited Primitive strata, including granite and other igneous and metamorphic rocks. On these followed the Transition rocks, the earliest sediments of mechanical origin, and above them the Floetz rocks, a term for the horizontal stratified rocks. These last he said were partly of chemical but chiefly of mechanical origin. Last of all came the Alluvial series.

The existence of volcanoes had been pointed out long before Werner's time by the Italian school of geologists, but as for "the universality and potency of what is now termed igneous action," all was "brushed aside by the oracle of Freiberg." Reactions between the interior and exterior of our earth "were utterly antagonistic to Werner's conception of the structure and history of the

earth." To him, volcanoes were "burning mountains" that arose from the combustion of subterranean beds of coal, spontaneously ignited.

The breaking down of the Wernerian doctrines began with two of Werner's most distinguished pupils, D'Aubuisson de Voisins (1769-1819) and Von Buch. The former in 1803 had accepted Werner's aqueous origin of basalt, but after studying the celebrated and quite recent volcanic area of Auvergne he recanted in 1804. Here he saw the basaltic rocks lying upon and cutting through granite, and in places more than 1200 feet thick. "If these basaltic rocks were lavas," says Geikie, "they must, according to the Wernerian doctrine, have resulted from the combustion of beds of coal. But how could coal be supposed to exist under granite, which was the first chemical precipitate of a primeval ocean?"

Leopold von Buch (1774-1853), "the most illustrious geologist that Germany has produced," after two years spent in Norway was satisfied "that the rocks in the Christiania district could not be arranged according to the Wernerian plan, which there completely broke down. Von Buch found a mass of granite lying among fossiliferous limestones which were manifestly metamorphosed, and were pierced by veins of granite, porphyry, and syenite." Even so, he was not ready to abandon the teachings of his master. After a study of the mountain systems of Germany, however, "he declared that the more elevated mountains had never been covered by the sea, as Werner had taught, but were produced by successive ruptures and uplifts of the terrestrial crust" (Geikie).

Rise of Geology and Conformism.—Modern geology has its rise in James Hutton (1726-1797) of Edinburgh, Scotland. In 1785 and 1795, Hutton published his Theory of the Earth, with Proofs and Illustrations. His "immortal theory" is his only work on geology. "Fortunately for Hutton's fame and for the onward march of geology, the philosopher numbered among his friends the illustrious mathematician and natural philosopher, John Playfair (1748-1819), who had been closely associated with him in his later years, and was intimately conversant with his geological opinions." In 1802, Play-

fair published his Illustrations of the Huttonian Theory of the Earth, of which Geikie says, "Of this great classic it is impossible to speak too highly," as it is at the basis of all modern geology.

One of Hutton's fundamental doctrines is that the earth is internally hot and that in the past large masses of molten material, the granites, have been intruded into the crust. It was these igneous views that led to his followers being called the Plutonists. Another of his great doctrines was that "the ruins of an earlier world lie beneath the secondary strata," and that they are separated by what is now known as unconformity. He clearly recognized a lost interval in the broken relation of the structures, and that the ruins, the detrital materials, of one world after another are superposed in the structure of the earth.

Hutton also held that the deformation of once horizontally deposited strata was probably brought about at different periods by great convulsions that shook the very foundations of the earth. After a convulsion, there was a long time of erosion, represented by the unconformity. Geikie says, "The whole of the modern doctrine of earth sculpture is to be found in the Huttonian theory."

The Lyellian doctrine of metamorphism had its origin in Hutton, for he showed that invading igneous granite had altered, through its heat and expanding power, the originally water-laid sediments, and that the schists of the Alps had been born of the sea like other stratified rocks.

Hutton is the father of the Uniformitarian principle, for he "started with the grand conception that the past history of our globe must be explained by what can be seen to be happening now, or to have happened only recently. The dominant idea in his philosophy is that the present is the key to the past." This principle has been impressed on all later geologists by Sir Charles Lyell, and is the chief cornerstone of modern geology.

The principle of uniformitarianism has underlain geologic interpretation since the days of Hutton, Playfair, and Lyell. However, it is often applied too rigidly in interpretations based upon the present conditions, because in the past there were long times when the topo-

graphic features of the earth were very different from those of to-day. Throughout the Paleozoic, and, less markedly, the Mesozoic, the oceans flooded the lands widely (at times over 60 per cent of the total area), highlands were inconspicuous, sediments far scarcer, and climates warm and equable throughout the world. Highland conditions, and especially the broadly emergent continents of the present, were only periodically present in the Paleozoic and then for comparatively short intervals between the periods. Therefore rates of denudation, solution, sedimentation, and evolution have varied greatly throughout the geological ages. These differences, however, relate to degrees of operation, and not to kinds of processes; but the differences in degree of operation react mightily on our views as to the age of the earth.

Geologic time had, for Hutton, no "vestige of a beginning, no prospect of an end." In other words, geologic time is infinite. He did not, however, discover a method by which the chronology of the earth could be determined.

First Important Text-books.—In 1822 appeared the ablest text-book so far published, and the pattern for most of the later ones, Outlines of the Geology of England and Wales, by W. D. Conybeare (1787-1857) and W. Phillips (1775-1828). "In this excellent volume all that was then known regarding the rocks of the country, from the youngest formations down to the Old Red Sandstone, was summarized in so clear and methodical a manner as to give a powerful impulse to the cultivation of geology in England" (Geikie). This book is reviewed at great length by Edward Hitchcock in the Journal (**7**, 203, 1824).

To indicate how far historical geology had progressed up to 1822 in England, a digest of the geological column as presented in this text-book is given in the following table, along with other information.

A text-book writer of yet greater influence was Charles Lyell (1797-1875), whose Principles of Geology appeared in three volumes between 1830 and 1833. This and his other books were kept up to date through many editions, and his Elements of Geology is, as Geikie says, "the hand book of every English geologist" working with the fossiliferous formations.

The Rise of Geology in North America.

The Generating Centers.—In America, geology had its rise independently in three places: in the two scientific societies of Boston and Philadelphia, and dominantly in Benjamin Silliman of Yale College. Stated in another way, we may say that geology in America had its origin in the following pioneers and founders: first, in William Maclure at Philadelphia, and next in Benjamin Silliman at New Haven. Through the influence of the latter, Amos Eaton, the botanist, became a geologist and taught geology at Williams College and later at the Rensselaer School in Troy, New York. Through the same influence Rev. Edward Hitchcock also became a geologist and taught the subject after 1825 at Amherst College.

Silliman was the first to take up actively the teaching of mineralogy and geology based on collections of specimens. He spread the knowledge in popular lectures throughout the Eastern States, graduated many a student in the sciences, making of some of them professional teachers and geologists, provided all with a journal wherein they could publish their research, organized the first geological society and through his students the first official geological surveys, and by kind words and acts stimulated, fostered, and held together American scientific men for fifty years. Of him it has been truly said that he was "the guardian of American science from its childhood."

The American Academy in Boston.—The second oldest scientific society, but the first one to publish on geological subjects, was the American Academy of Arts and Sciences of Boston, instituted and publishing since 1780. Up to the time of the founding of this Journal, there had appeared in the publications of the American Academy about a dozen papers of a geologic character, none of which need to be mentioned here excepting one by S. L. and J. F. Dana, entitled "Outlines of the Mineralogy and Geology of Boston," published in 1818. This is an early and important step in the elucidation of one of the most intricate geologic areas, and is further noteworthy for its geologic map, the third one to appear, the older ones being by Maclure and Hitchcock (Merrill).

The Geological Column in 1822

<table>
<tr><th colspan="2">Present American classification</th><th>Conybeare and Phillips 1822</th><th>C.&P. orders</th><th>Wernerian orders</th><th>Other writers</th></tr>
<tr><td colspan="2">Psychozoic or Recent</td><td>Alluvial</td><td rowspan="4">Superior Order</td><td rowspan="4">Newest Floetz Class</td><td rowspan="4">Tertiary Class</td></tr>
<tr><td rowspan="3">Cenozoic</td><td>Pleistocene</td><td>Diluvial</td></tr>
<tr><td>Pliocene, Miocene } Neogene</td><td>Upper Marine formation (Crag, Bagshot sand, and Isle of Wight)
Freshwater formations</td></tr>
<tr><td>Oligocene, Eocene } Paleogene</td><td>London Clay
Plastic Clay</td></tr>
<tr><td rowspan="4">Mesozoic</td><td>Cretaceous</td><td>Chalk</td><td rowspan="5">Supermedial Order</td><td rowspan="7">Floetz Classes and Transition</td><td rowspan="7">Secondary Classes and Intermediate</td></tr>
<tr><td>Comanchian 1887</td><td>Beds between Chalk and Oolite Series (Chalk Marle, Green Sand, Weald Clay, Iron Sand)</td></tr>
<tr><td>Jurassic 1829</td><td>Upper Oolitic division (Purbeck beds, Portland Oolite, Kimmeridge Clay)
Middle Oolitic division (Coral Rag, Oxford Clay)
Lower Oolitic division (Cornbrash, Stonesfield Slate, Forest Marble, Great Oolite, Fullers' Earth, Inferior Oolite, Sand and Marlestone
Lias</td></tr>
<tr><td>Triassic 1834</td><td>New Red Sandstone</td></tr>
<tr><td rowspan="3">Paleozoic</td><td>Permian 1841</td><td>Magnesian Limestone</td></tr>
<tr><td>Pennsylvanian 1891
Mississippian 1869</td><td>Coal Measures
Millstone Grit and Shale
Old Red Sandstone</td><td>Medial or Carboniferous Order</td></tr>
<tr><td>Devonian 1839
Silurian 1835
Ordovician 1879
(=Lower Silurian 1835)
Cambrian 1833</td><td colspan="2" rowspan="3">Unresolved
Submedial
and
Inferior Orders</td></tr>
<tr><td>Proterozoic</td><td>Keweenawan, Animikian, Huronian, Sudburian } Huronian 1852</td><td rowspan="2">Primitive</td><td rowspan="2">Primitive</td></tr>
<tr><td>Archeozoic</td><td>Keewatin, Coutchiching } Laurentian 1853</td></tr>
</table>

Early Geology in Philadelphia.—The oldest scientific society is the American Philosophical Society of Philadelphia, started by the many-sided Benjamin Franklin in 1769, and which has published since 1771. Up to the time of the founding of the Journal in 1818, there had appeared in the publications of this society thirteen papers of a geologic nature, nearly all small building stones in the rising geologic story of North America. The only fundamental ones were Maclure's Observations of 1809 and 1817. Later, in this same city, there was organized another scientific society that came to be for a long time the most active one in America. This was the Academy of Natural Sciences, started in 1812 with seven members, but it was not until 1817 and the election of William Maclure as its first president that the work of the Academy was of a far-reaching character. Here was built up not only a society for the advancement of the natural sciences and publications for the dissemination of such knowledge, but, what is equally important, the first large library and general museum.

William Maclure (1763-1840), correctly named by Silliman the "father of American geology," was born and educated in Scotland, and died near Mexico City. A merchant of London until 1796, when he had already amassed "a considerable fortune," he made a first short visit to New York City in 1782. In 1796 he again came to America, this time to become a citizen of this country and a liberal patron of science.

About 1803, single-handed and unsustained by government patronage, Maclure interested himself most zealously and efficiently in American geology. In 1809 he published his Observations on the Geology of the United States, Explanatory of a Geological Map. This work he revised "on a yet more extended scale," issuing it in 1817 with 130 pages of text, accompanied by a large colored geological map.

Silliman, the Pioneer Promoter of Geology.—In 1806 when Benjamin Silliman (1779-1864) began actively to teach chemistry and mineralogy, all the sciences in America were in a very backward state, and the earth sciences were not recognized as such in the curricula of any of our colleges. Silliman gave his first lecture in chemistry on

April 4, 1804. In the summer of that year, Yale College asked him to go to England to purchase material for the College, and great possibilities for broadening his knowledge now loomed before him. As Silliman himself (**43**, 225, 1842) has told the interesting story of his sojourn in England and Scotland, it is worth while to restate a part of it here.

"Passing over to England in the spring of 1805, and fixing my residence for six months in London, I found there no school, public or private, for geological instruction, and no association for the cultivation of the science, which was not even named in the English universities." In geology "Edinburgh was then far in advance of London . . . Prof. Jameson having recently returned from the school of Werner, fully instructed in the doctrines of his illustrious teacher, was ardently engaged to maintain them, and his eloquent and acute friend, the late Dr. John Murray, was a powerful auxiliary in the same cause; both of these philosophers strenuously maintaining the ascendancy of the aqueous over the igneous agencies, in the geological phenomena of our planet.

On the other hand, the disciples and friends of Dr. Hutton were not less active. He died in 1797, and his mantle fell upon Sir James Hall, who, with Prof. Playfair and Prof. Thomas Hope, maintained with signal ability, the igneous theory of Hutton. It did not become one who was still a youth and a novice, to enter the arena of the geological tournament where such powerful champions waged war; but it was very interesting to view the combat, well sustained as it was on both sides, and protracted, without a decisive issue, into a drawn battle . . .

The conflicts of the rival schools of Edinburgh—the Neptunists and the Vulcanists, the Wernerians and the Huttonians, were sustained with great zeal, energy, talent, and science; they were indeed marked too decidedly by a partisan spirit, but this very spirit excited untiring activity in discovering, arranging, and criticising the facts of geology. It was a transition period between the epoch of geological hypotheses and dreams, which had passed by, and the era of strict philosophical induction, in which the geologists of the present day are trained . . .

I was a diligent and delighted listener to the discussions of both schools. Still the igneous philosophers appeared to me to assume more than had been proved regarding internal heat. In imagination we were plunged into a fiery Phlegethon, and I was glad to find relief in the cold bath of the Wernerian ocean, where my predilections inclined me to linger."

Silliman's Students and Their Publications.—Silliman's first student to take up geology as a profession was Denison Olmstead (1791-1859), educator, chemist, and geologist, who was graduated from Yale in 1813. Four years later he was under special preparation with Silliman in mineralogy and geology, and in that year was appointed professor of chemistry in the University of North Carolina. In 1824-1825 Olmstead issued a Report on the Geology of North Carolina, which is the first official geological report issued by any state in America, "a conspicuous and solitary instance," according to Hitchcock's review of it (**14**, 230, 1828), "in which any of our state governments have undertaken thoroughly to develop their mineral resources."

Amos Eaton (1776-1842), lawyer, botanist, surveyor, and one of the founders of American geology, was a graduate of Williams College in the class of 1799. He studied with Silliman in 1815, attending his lectures on chemistry, geology, and mineralogy. He also enjoyed access to the libraries of Silliman and of the botanist, Levi Ives, in which works on botany and materia medica were prominent, and was a diligent student of the College cabinet of minerals. He settled as a lawyer and land agent in Catskill, New York, and here in 1810 he gave a popular course of lectures on botany, believed to have been the first attempted in the United States.

In 1818 appeared Eaton's first noteworthy geological publication, the Index to the Geology of the Northern States, a text-book for the classes in geology at Williamstown. The controlling principle of this book was Wernerism, a false doctrine from which Eaton was never able to free himself. This book was "written over anew" and published in 1820.

While at Albany in 1818, Governor De Witt Clinton asked Eaton to deliver a course of lectures on chemistry and geology before the members of the legislature of New York. It is believed that Eaton is the only American having this distinction, and because of it he became acquainted with many leading men of the state, interesting them in geology and its application to agriculture by means of surveys. In this way was sown the idea

which eventually was to fructify in that great official work: The Natural History of New York. (See **43**, 215, 1842; and Youmans' sketch of Eaton's life, Pop. Sci. Monthly, Nov. 1890.)

Edward Hitchcock (1793-1864), reverend, state geologist, college president, and another of the founders of American geology, was largely self-taught. Previous to 1825, when he entered the theological department of Yale College, he had met Amos Eaton, who interested him in botany and mineralogy, and between 1815 and 1819 he had made lists of the plants and minerals found about his native town, Deerfield, Massachusetts. Therefore, while studying theology at Yale it was natural for him also to take up mineralogy and geology with Silliman, whose acquaintance he had made at least as early as 1818.

Hitchcock, who was destined to be one of the most prominent figures of his time, was appointed in 1825 to the chair of chemistry and natural history at Amherst College. His first geologic paper, one of five pages, appeared in 1815. Three years later appeared his more important paper on the Geology and Mineralogy of a Section of Massachusetts, New Hampshire, and Vermont (**1**, 105, 436, 1818). This is also noteworthy for its geological map, the next one to be published after those of Maclure of 1809 and 1817. In 1823 came a still greater work, A Sketch of the Geology, Mineralogy, and Scenery of the Regions contiguous to the River Connecticut (**6**, 1, 200, 1823; **7**, 1, 1824). Here the map above referred to was greatly improved, and the survey was one of the most important of the older publications.

Youmans in his account of Hitchcock (Pop. Sci. Monthly, Sept. 1895) says:

"The State of Massachusetts commissioned him to make a geological survey of her territory in 1830. Three years were spent in the explorations, and the work was of such a high character that other States were induced to follow the example of Massachusetts . . . The State of New York sought his advice in the organization of a survey, and followed his suggestions, particularly in the division of the territory into four parts, and appointed him as the geologist of the first district. He entered upon the work, but after a few days of labor he found that he must necessarily be separated from his family, much to his dis-

inclination. He also conceived the idea of urging a more thorough survey of his own State; hence he resigned his commission and returned home. The effort for a resurvey of Massachusetts was successful, and he was recommissioned to do the work. The results appeared in 1841 and 1844."

Oliver P. Hubbard was assistant to Silliman in 1831-1836, and then up to 1866 taught chemistry, mineralogy, and geology at Dartmouth College. James G. Percival was graduated at Yale in 1815, and in 1835 he and C. U. Shepard of Amherst College were appointed state geologists of Connecticut. Their report was issued in 1842.

James Dwight Dana (1813-1895) was undoubtedly the ablest of all of Silliman's students. Graduated at Yale in 1833, he spent fifteen months in the United States Navy as instructor in mathematics, cruising off France, Italy, Greece, and Turkey. In 1836 he was assistant to Silliman, and in 1837, at the age of twenty-four years, he published his widely used System of Mineralogy. Two years later Dana joined the Wilkes Exploring Expedition as mineralogist, returning to America in 1842; his geological results of this expedition were published in 1849. In 1863, during the Rebellion, he published his Manual of Geology, and through four editions it remained for forty years the standard text-book for American geologists.

First American Geological Society.—The founding in 1807 of the Geological Society of London, the parent of geological societies, undoubtedly had its stimulating effect on Silliman, and with his marked organizing ability he began to think of forming an American society of the same kind. This he brought about the year following the appearance of the Journal, that is, in 1819. The American Geological Society, begun in 1819 (**1**, 442, 1819), was terminated in 1830 (**17**, 202, 1830). The first meeting (September 6, 1819) and all the subsequent ones were held in the cabinet of Yale College. The brief records of the doings of this society are printed in volumes **1**, **10**, **15**, and **18** of the Journal. Silliman was the attraction at the meetings, surrounded by his mineral cabinet, and he gave "the true scientific dress to all the naked mineralogical subjects" discussed.

Wernerian Geology in North America.

The Father of American Geology.—Historical Geology begins in America with William Maclure's Observations on the Geology of the United States, issued in 1809. This was the first important original work on North American geology, and its colored geological map was the first one of the area east of the Mississippi River. The classification was essentially the Wernerian system. All of the strata of the Coastal Plain, now known to range from the Lower Cretaceous to Recent, were referred to the Alluvial. To the west, over the area of the Piedmont, were his Primitive rocks, while the older Paleozoic formations of the Appalachian ranges were referred to the Transition. West of the folded area, all was Floetz or Secondary, or what we now know as Paleozoic sedimentaries. The Triassic of the Piedmont area and that of Connecticut he called the Old Red Sandstone, and the coal formations of the interior region he said rested upon the Secondary. The second edition of the work in 1817 was much improved, along with the map, which was also printed on a more correct geographic base. (For greater detail, see Merrill, Contributions to the History of American Geology, 1906.)

Even though Maclure's geologic maps are much generalized, and the scheme of classification adopted a very broad one, they are in the main correct, even if they do emphasize unduly the rather simple geologic structure of North America. This fact is patent all through Maclure's description. Cleaveland also refers to it in his treatise of 1816, and Silliman in the opening volume of the Journal (**1**, 7, 1818) says: "The outlines of Amercan geology appear to be particularly grand, simple, and instructive." Then, all the kinds of rocks were comprehended under four classes, Primitive, Transition, Alluvial, and Volcanic. It is also interesting to note here that in 1822 Maclure had lost faith in the aqueous origin of the igneous rocks and writes of the Wernerian system as "fast going out of fashion" (**5**, 197, 1822), while Hitchcock said about the same thing in 1825 (**9**, 146).

The Work of Eaton.—Amos Eaton, after traveling 10,000 miles and completing his Erie Canal Report in

1824, "reviewed the whole line several times," and published in 1828 in the Journal (**14**, 145) a paper on Geological Nomenclature; Classes of Rocks, etc. The broader classification is the Wernerian one of Primitive, Transition, and Secondary classes. Under the first two he has fossiliferous early Paleozoic formations, but does not know it, because he pays no attention anywhere to the detail of the entombed fossils, and all of his Secondary is what we now call Paleozoic. The correlations of the latter are faulty throughout.

Then came his paper of 1830, Geological Prodromus (**17**, 63), in which he says: "I intend to demonstrate . . . that all geological strata are arranged in five analogous series; and that each series consists of three formations; viz., the Carboniferous [meaning mud-stones], Quartzose, and Calcareous." We seem to see here expressed for the first time the idea of "cycles of sedimentation," but Eaton does not emphasize this idea, and the localities given for each "formation" of "analogous series" demonstrate beyond a doubt that he did not have a sedimentary sequence. The whole is simply a jumble of unrelated formations that happen to agree more or less in their physical characters.

"I intend to demonstrate," he says further, "that the detritus of New Jersey, embracing the marle, which contains those remarkable fossil relics, is antediluvial, or the genuine Tertiary formation." This correlation had been clearly shown by Finch in 1824 (**7**, 31) and yet both are in error in that they do not distinguish the included Cretaceous marls and greensands as something apart from the Tertiary.

One gets impatient with the later writings of Eaton, because he does not become liberalized with the progressive ideas in stratigraphic geology developing first in Europe and then in America, especially among the geologists of Philadelphia. Therefore it is not profitable to follow his work further.

Early American Text-books of Geology.—The first American text-book of geology bears the date of Boston 1816 and is entitled An Elementary Treatise on Mineralogy and Geology, its author being Parker Cleaveland of Bowdoin College. The second edition appeared in 1822.

It also had a geologic map of the United States, practically a copy of Maclure's. To mineralogy were devoted 585 pages, and to geology 55, of which 37 describe rocks and 5 the geology of the United States. The chronology is Wernerian. Of "geological systems" there are two, "primitive and secondary rocks."

In 1818 appeared Amos Eaton's Index to the Geology of the Northern States, having 54 pages, and in 1820 came the second edition, "wholly written over anew," with 286 pages. The theory of the later edition is still that of Werner, with "improvements of Cuvier and Bakewell," and yet one sees nowadays but little in it of the far better English text-book. Eaton did very little to advance philosophic geology in America. What is of most value here are his personal observations in regard to the local geology of western Massachusetts, Connecticut, southwestern Vermont, and eastern New York (**1**, 69, 1819; also Merrill, p. 234).

We come now to the most comprehensive and advanced of the early text-books used in America. This is the third English edition of Robert Bakewell's Introduction to Geology (400 pages, 1829), and the first American edition "with an Appendix Containing an Outline of his Course of Lectures on Geology at Yale College, by Benjamin Silliman" (128 pages). Bakewell's good book is in keeping with the time, and while not so advanced as Conybeare and Phillips's Outlines of 1822, yet is far more so than Silliman's appendix. The latter is general and not specific as to details; it is still decidedly Wernerian, though in a modified form. Silliman says he is "neither Wernerian nor Huttonian," and yet his summary on pages 120 to 126 shows clearly that he was not only a Wernerian but a pietist as well.

Unearthing of the Cenozoic and Mesozoic in North America.

The Discerning of the Tertiary.—The New England States, with their essentially igneous and metamorphic formations, could not furnish the proper geologic environment for the development of stratigraphers and paleontologists. So in America we see the rise of such geologists first in Philadelphia, where they had easy

access to the horizontal and highly fossiliferous strata of the coastal plain. The first one to attract attention was Thomas Say, after him came John Finch, followed by Lardner Vanuxem, Isaac Lea, Samuel G. Morton, and T. A. Conrad. These men not only worked out the succession of the Cenozoic and the upper part of the Mesozoic, but blazed the way among the Paleozoic strata as well.

Thomas Say (1787-1834), in 1819, was the first American to point out the chronogenetic value of fossils in his article, Observations on some Species of Zoophytes, Shells, etc., principally Fossil (1, 381). He correctly states that the progress of geology "must be in part founded on a knowledge of the different genera and species of reliquiæ, which the various accessible strata of the earth present." Say fully realizes the difficulties in the study of fossils, because of their fragmental character and changed nature, and that their correct interpretation requires a knowledge of similar living organisms.

The application of what Say pointed out came first in John Finch's Geological Essay on the Tertiary Formations in America (7, 31, 1824). Even though the paper is still laboring under the mineral system and does not discern the presence of Cretaceous strata among his Tertiary formations, yet Finch also sees that "fossils constitute the medals of the ancient world, by which to ascertain the various periods."

Finch now objects to the wide misuse in America of the term alluvial and holds that it is applied to what is elsewhere known as Tertiary. He says:

> "Geology will achieve a triumph in America, when the term alluvial shall be banished from her Geological Essays, or confined to its legitimate domain, and then her tertiary formations will be seen to coincide with those of Europe, and the formations of London, Paris, and the Isle of Wight, will find kindred associations in Virginia, the Carolinas, Georgias, the Floridas, and Louisiana."

The formations as he has them from the bottom upwards are: (1) Ferruginous sand, (2) Plastic clay, (3) Calcaire Silicieuse of the Paris Basin, (4) London Clay, (5) Calcaire Ostrée, (6) Upper marine formation, (7) Diluvial.

The grandest of these early stratigraphic papers, however, is that by Lardner Vanuxem (1792-1848), of only three pages, entitled "Remarks on the Characters and Classification of Certain American Rock Formations" (**16**, 254, 1829). Vanuxem, a cautious man and a profound thinker, had been educated at the Paris School of Mines. James Hall told the writer in a conversation that while the first New York State Survey was in operation, all of its members looked to Vanuxem for advice.

In the paper above referred to, Vanuxem points out in a very concise manner that:

"The alluvial of Mr. Maclure . . . contains not only well characterized alluvion, but products of the tertiary and secondary classes. Littoral shells, similar to those of the English and Paris basins, and pelagic shells, similar to those of the chalk deposition or latest secondary, abound in it. These two kinds of shells are not mixed with each other; they occur in different earthy matter, and, in the southern states particularly, are at different levels. The incoherency or earthiness of the mass, and our former ignorance of the true position of the shells, have been the sources of our erroneous views."

The second error of the older geologists, according to Vanuxem, was the extension of the secondary rocks over "the western country, and the back and upper parts of New York." They are now called Paleozoic. Some had even tried to show the presence of Jurassic here because of the existence of oölite strata. "It was taken for granted, that all horizontal rocks are secondary, and as the rocks of these parts of the United States are horizontal in their position, so they were supposed to be secondary." He then shows on the basis of similar Ordovician fossils that the rocks of Trenton Falls, New York, recur at Frankfort in Kentucky, and at Nashville in Tennessee.

"It is also certain that an uplifting or downfalling force, or both, have existed, but it is not certain that either or both these forces have acted in a uniform manner. . . . Innumerable are the facts, which have fallen under my observation, which show the fallacy of adopting inclination for the character of a class," such as the Transition class of strata. He then goes on to say that in the interior of our country the so-called secondary rocks are horizontal and in the mountains to the east the

same strata are highly inclined. "The analogy, or identity of rocks, I determine by their fossils in the first instance, and their position and mineralogical characters in the second or last instance."

It appears that Isaac Lea (1792-1886) in his Contributions to Geology, 1833, was the first to transplant to America Lyell's terms, Pliocene, Miocene, and Eocene, proposed the previous year. The celebrated Claiborne locality was made known to Lea in 1829, and in the work here cited he describes from it 250 species, of which 200 are new. The horizon is correlated with the London Clay and with the Calcaire Grossier of France, both of Eocene time (**25**, 413, 1834).

Timothy A. Conrad began to write about the American Tertiary in 1830, and his more important publications were issued at Philadelphia. His papers in the Journal begin with 1833 and the last one on the Tertiary is in 1846.

The Tertiary faunas and stratigraphy have been modernized by William H. Dall in his monumental work of 1650 pages and 60 plates entitled "Contributions to the Tertiary Fauna of Florida" (1885-1903). Here more than 3160 forms of the Atlantic and Gulf deposits are described, but in order to understand their relations to the fossil faunas elsewhere and to the living world, the author studied over 10,000 species. Since then, many other workers have interested themselves in the Tertiary problems. Much good work is also being done in the Pacific States where the sequence is being rapidly developed.

The Discerning of the Eastern Cretaceous.—The Cretaceous sequence was first determined by that "active and acute geologist," Samuel G. Morton (1799-1851), but that these rocks might be present along the Atlantic border had been surmised as early as 1824 by Edward Hitchcock (**7**, 216). Vanuxem, as above pointed out, indicated the presence of the Cretaceous in 1829. In this same year Morton proved its presence before the Philadelphia Academy of Natural Sciences.

Between 1830 and 1835 Morton published a series of papers in the Journal under the title "Synopsis of the Organic Remains of the Ferruginous Sand Formation of

the United States, with Geological Remarks" (**17**, 274, *et seq.*). In these he describes the Cretaceous fossils and demonstrates that the "Diluvial" and Tertiary strata of the Atlantic border also have a long sequence of Cretaceous formations. In the opening paper he writes: "I consider the marl of New Jersey as referable to the great ferruginous sand series, which in Prof. Buckland's arrangement is designated by the name of green sand. . . . On the continent this series is called the ancient chalk . . . lower chalk," etc. Again, the marls of New Jersey are "geologically equivalent to those beds which in Europe are interposed between the white chalk and the Oölites." This correlation is with the European Lower Cretaceous, but we now know the marls to be of Upper Cretaceous age. Although Eaton objected strenuously to Morton's correlation, we find M. Dufresnoy of France saying, "Your limestone above green sand reminds me very much of the Mæstricht beds," a correlation which stands to this day (**22**, 94, 1832). In 1833 Morton announces that the Cretaceous is known all along the Atlantic and Gulf border, and in the Mississippi valley. "The same species of fossils are found throughout," and none of them are known in the Tertiary. He now arranges the strata of the former "Alluvial" as follows:

Modern	Alluvial. Diluvial.	
Tertiary	Upper Tertiary (Upper Marine). Middle Tertiary (London Clay). Lower Tertiary (Plastic Clay).	
Secondary	Calcareous Strata Ferruginous Sand	Cretaceous group, or Ferruginous Sand series (**24**, 128).

Western Cretaceous.—In 1841 and 1843 J. N. Nicollet announced the discovery of Cretaceous in the Rocky Mountain area. Of 20 species of fossils collected by him, 4 were said to occur on the Atlantic border, and of the 200 forms of the Atlantic slope only 1 was found in Europe. Here we see pointed out a specific dissimilarity between the continents, and a similarity between the American areas of Cretaceous deposits (**41**, 181; **45**, 153).

The Cretaceous of the Rocky Mountains was clearly

developed by F. V. Hayden in 1855-1888 and by F. B. Meek (1857-1876). Other workers in this field were Charles A. White (1869-1891), and R. P Whitfield (1877-1889). Since 1891 T. W. Stanton has been actively interpreting its stratigraphy and faunas.

Cretaceous and Comanche of Texas.—The broader outlines of the Cretaceous of Texas had been described by Ferdinand Roemer in 1852 in his good work, Kreidebildungen von Texas, but it was not until 1887 that Robert T. Hill showed in the Journal (**33**, 291) that it included two great series, the Gulf series, or what we now call Upper Cretaceous, and a new one, the Comanche series. This was a very important step in the right direction. Since then the Comanche series has been regarded by some stratigraphers as of period value, while others call it Lower Cretaceous; the rest of the Texas Cretaceous is divided by Hill into Middle and Upper Cretaceous. On the other hand, Lower Cretaceous strata had been proved even earlier in the state of California, for here in 1869 W. M. Gabb (1839-1878) and J. D. Whitney (1819-1896) had defined their Shasta group, which was wholly distinct faunally from the Comanche of Texas and the southern part of the Great Plains country.

Jurassic and Triassic of the West.—In 1864, the Geological Survey of California proved the presence of marine Upper Triassic in that State, and since then it has been shown that not only is all of the Triassic present in Idaho (where it has been known since 1877), Oregon, Nevada, and California, but that the Upper Triassic is of very wide distribution throughout western North America. Jurassic strata, on the other hand, were not shown to be present in California until 1885, while in the Rocky Mountain area of the United States there was long known an unresolved series of "Red Beds" situated between the Carboniferous and Cretaceous. This gave rise to the "Red Bed problem," the history of which is given by C. A. White in the Journal (**17**, 214, 1879). In 1869, F. V. Hayden announced the discovery of marine Jurassic fossils in this series, and since then they have come to be known as the Sundance fauna, extending from southern Utah and Colorado into Alaska.

Above lie the dinosaur-bearing fresh-water deposits, since 1894 known as the Morrison beds. In 1896, O. C. Marsh (1831-1899) announced the presence of Jurassic fresh-water strata along the Atlantic coast (**2**, 433), but to-day only a small part of them are regarded as of the age of the Morrison, while the far greater part are referred to the Comanche or Lower Cretaceous. The red beds below the Jurassic of the Rocky Mountain area have during the past twenty years been shown to be in part of Upper Triassic age and of fresh-water origin, while the greater lower part is connected with the Carboniferous series and is made up of brackish- and fresh-water deposits of probable Permian time.

Triassic of Atlantic States.—The fresh-water Triassic of the Atlantic border states was first mentioned by Maclure (1817), who regarded it as the equivalent of the Old Red Sandstone of Europe. In this he was followed by Hitchcock in 1823 (**6**, 39), the latter saying that above it lies "the coal formation," which is true for Europe, but in America the coal strata are older than these red beds, now known to be of Triassic age.

The first one to question this correlation was Alexandre Brongniart, who had received from Hitchcock rock specimens and a fossil fish which he erroneously identified with a Permian species, and accordingly referred the strata to the Permian (**3**, 220, 1821; **6**, 76, pl. 9, figs. 1, 2, 1823). The discerning Professor Finch in 1826 remarked that the red beds of Connecticut appear to belong "to the new or variegated sandstone," because of eight different criteria that he mentions. Of these, but two are of value in correlation, their "geological position" and the presence of bones other than fishes. In the Connecticut area, however, the geological position cannot be determined even to-day, and in Finch's time the bones of dinosaurs were unknown. Finch then goes on to point out the occurrences of Old Red Sandstone in Pennsylvania, but all of the places he refers to are either younger or older in time. Here we again see the fatality of trying to make positive correlations on the basis of lithology and color (**10**, 209, 1826). In 1835, however, Hitchcock showed that the bones that had been found in 1820 were those of a saurian, and accordingly referred

the strata of the Connecticut valley to the New Red Sandstone, a term that then covered both the Permian and the Triassic. In 1842, W. B. Rogers referred the beds to the Jurassic, on the basis of plants from Virginia. In 1856, W. C. Redfield (1789-1857), because of the fishes, advocated a Lias, or Jurassic age, and proposed the name Newark group for all the Triassic deposits of the Atlantic border. More recently, on the basis of the plants studied by Newberry, Fontaine, Sturr, and Ward, and the vertebrates described by Marsh and Lull, the age has been definitely fixed as Upper Triassic (see Dana's Manual of Geology, 740, 1895).

Unearthing of the Paleozoic in North America.

Permian of the United States.—In Europe, previous to 1841, the formations now classed as Permian were included in the New Red Sandstone, and with the Carboniferous were referred to the Secondary. In that year Murchison proposed the period term Permian. In 1845 came the classic Geology of Russia in Europe and the Ural Mountains, by Murchison, Keyserling, and De Verneuil. In this great work the authors separated out of the New Red the Magnesian Limestone of Great Britain and the Rothliegende marls, Kupferschiefer, and Zechstein of Germany, and with other formations of the Urals in Russia, referred them to the Permian system. This step, one of the most discerning in historical geology, was all the more important because they closed the Paleozoic era with the Permian, beginning the Secondary, or Mesozoic, with the New Red Sandstone or the Triassic period. There is a good review of this work by D. D. Owen (1807-1860) in the Journal for 1847 (**3**, 153).

Owen, though accepting the Permian system, is not satisfied with its reference to the Paleozoic, and he sets the matter forth in the Journal (**3**, 365, 1847). He doubts "the propriety of a classification which throws the Permian and Carboniferous systems into the Paleozoic period." This is mainly because there is no "evidence of disturbance or unconformability" between the Permian and Triassic systems. Rather "there is so complete a blending of adjacent strata" that it is only

in Russia that the Permian has been distinguished from the Triassic. This view of Owen's was not only correct for Russia but even more so for the Alps and for India, and it has taken a great deal of work and discussion to fix upon the disconformable contact that distinguishes the Paleozoic from the Mesozoic in these areas. In other words, there was here at this time no mountain making. Then Owen goes on to state that because the Permian of Europe has reptiles, he sees in them decisive Mesozoic evidence. "These are certainly strong arguments in favor of placing, not only the Permian, but also the Carboniferous group in the Mesozoic period, and terminating the Paleozoic division with the commencement of the coal measures." To this harking backward the geologists of the world have not agreed, but have followed the better views of Murchison and his associates.

In 1855 G. G. Shumard discovered, and in 1860 his brother B. F. Shumard (1820-1869) announced, the presence of Permian strata in the Guadalupe Mountains of Texas, and in 1902 George H. Girty (**14**, 363) confirmed this. Girty regards the faunas as younger than any other late Paleozoic ones of America, and says: "For this reason I propose to give them a regional name, which shall be employed in a force similar to Mississippian and Pennsylvanian. . . . The term Guadalupian is suggested."

G. C. Swallow (1817-1899) in 1858 was the first to announce the presence of Permian fossils in Kansas, and this led to a controversy between himself and F. B. Meek, both claiming the discovery. It is only in more recent years that it has been generally admitted that there is Permian in that state, in Oklahoma, and in Texas. This admission came the more readily through the discovery of many reptiles in the red beds of Texas, and through the work of C. A. White, published in 1891, The Texan Permian and its Mesozoic Types of Fossils (Bull. U. S. Geological Survey, No. **77**).

Carboniferous Formations.—The coal formations are noted in a general way throughout the earliest volumes of the Journal. The first accounts of the presence of coal, in Ohio, are by Caleb Atwater (**1**, 227, 239, 1819), and S. P. Hildreth (**13**, 38, 40, 1828). The first coal

James Hall

plants to be described and illustrated were also from Ohio, in an article by Ebenezer Granger in 1821 (**3**, 5-7). The anthracite field was first described in 1822 by Zachariah Cist (**4**, 1) and then by Benjamin Silliman (**10**, 331-351, 1826); that of western Pennsylvania was described by William Meade in 1828 (**13**, 32).

The Lower Carboniferous was first recognized by W. W. Mather in 1838 (**34**, 356). Later, through the work of Alexander Winchell (1824-1891), beginning in 1862 (**33**, 352) and continuing until 1871, and through the surveys of Iowa (1855-1858), Illinois (essentially the work of A. H. Worthen, 1858-1888), Ohio (1838, Mather, etc.), and Indiana (Owen, etc., 1838), there was eventually worked out the following succession:

Permian period.
- Upper Barren series.
  - Dunkard group.
  - Washington group.

Pennsylvanian period.
- Upper Productive Coal series. Monongahela series.
- Lower Barren Coal Measures. Conemaugh series.
- Lower Productive Coal Measures. Allegheny series.
- Pottsville series.

The New York System.—We now come to the epochal survey of the State of New York, one that established the principles of, and put order into, American stratigraphy from the Upper Cambrian to the top of the Devonian. No better area could have been selected for the establishing of this sequence. This survey also developed a stratigraphic nomenclature based on New York localities and rock exposures, and made full use of the entombed fossils in correlation. Incidentally it developed and brought into prominence James Hall, who continued the stratigraphic work so well begun and who also laid the foundation for paleontology in America, becoming its leading invertebrate worker.

This work is reviewed at great length in the Journal in the volumes for 1844-1847 by D. D. Owen. Evidently it followed too new a plan to receive fulsome praise from conservative Owen, as it should have. He remarks that the volumes "are not a little prolix, are voluminous and

expensive, and do not give as clear and connected a view of the geological features of the state as could be wished. . . . We are of the opinion that before this work can become generally useful and extensively circulated, it must be condensed and arranged into one compendious volume" (**46**, 144, 1844). This was never done and yet the work was everywhere accepted at once, and to this end undoubtedly Owen's detailed review helped much.

The Natural History Survey of New York was organized in 1836 and completed in 1843. The state was divided into four districts, and to these were finally assigned the following experienced geologists. The southeastern part was named the First District, with W. W. Mather (1804-1859) as geologist; the northeastern quarter was the Second District, with Ebenezer Emmons (1799-1863) in charge; the central portion was the Third District, under Lardner Vanuxem (1792-1848); while the western part was James Hall's (1811-1898) Fourth District. Paleontology for a time was in charge of T. A. Conrad (1830-1877); the mineralogical and chemical work was in the hands of Lewis C. Beck; the botanist was John Torrey; and the zoologist James DeKay.

The New York State Survey published six annual reports of 1675 pages octavo, and four final geological reports with 2079 pages quarto. Finally in 1846 Emmons added another volume on the soils and rocks of the state, in which he also discussed the Taconic and New York systems; it has 371 pages. With the completion of the first survey, Hall took up his life work under the auspices of the state—his monumental work, Paleontology of New York, in fifteen quarto volumes of 4539 pages and 1081 plates of fossils. In addition to all this, there are his annual and other reports to the Regents of the State, so that it is safe to say that he published not less than 10,000 pages of printed matter on the geology and paleontology of North America.

In regard to this great series of works, all that can be presented here is a table of formations as developed by the New York State Survey. Practically all of its results and formation names have come into general use, with the exception of the Taconic system of Emmons and the division terms of the New York system. (See p. 88.)

The New York State Survey, begun in 1836, was continued by James Hall from 1843 to 1898. During this time he was also state geologist of Iowa (1855-1858) and Michigan (1862). Since 1898, John M. Clarke has ably continued the Geological Survey of New York, the state which continues to be, in science and more especially in geology and paleontology, the foremost in America.

Western Extension of the New York system.—Before Hall finished his final report, we find him in 1841 on "a tour of exploration through the states of Ohio, Indiana, Illinois, a part of Michigan, Kentucky, and Missouri, and the territories of Iowa and Wisconsin." This tour is described in the Journal (**42**, 51, 1842) under the caption "Notes upon the Geology of the Western States." His object was to ascertain how far the New York system as the standard of reference "was applicable in the western extension of the series." In a general way he was very successful in extending the system to the Mississippi River, and he clearly saw "a great diminution, first of sandy matter, and next of shale, as we go westward, and in the whole, a great increase of calcareous matter in the same direction." He also clearly noted the warped nature of the strata, the "anticlinal axis," since known as the Cincinnati and Wabash uplifts and the Ozark dome.

Hall, however, fell into a number of flagrant errors because of a too great reliance on lithologic correlation and supposedly similar sequence. For instance, the Coal Measures of Pennsylvania were said to directly overlap the Chemung group of southern New York, and now he finds the same condition in Ohio, Indiana, and Illinois, failing to see that in most places between the top of the New York system and the Coal Measures lay the extensive Mississippian series, one that he generally confounded with the Chemung, or included in the "Carboniferous group." He states that the Portage of New York is the same as the Waverly of Ohio, and at Louisville the Middle Devonian waterlime is correlated with the similar rock of the New York Silurian. Hall was especially desirous of fixing the horizon of the Middle Ordovician lead-bearing rocks of Illinois, Wisconsin, and Iowa, but unfortunately correlated them with the Niag-

The Geological Column of the New York Geologists of 1842–1843, according to W. W. Mather 1842.

Quaternary system	Alluvial division. Quaternary division. Drift division.	
Tertiary system	These strata are included in the next lower division.	
Upper Secondary system	Long Island division. Equals the Tertiary and Cretaceous marls, sands, and clays of the coastal plain of New Jersey.	
	Trappean division. The Palisades Red Sandstone division.	New Red system of Emmons and Hall.

Coal system of Mather, and Carboniferous system of Hall.

Old Red system of Catskill Mountains of Emmons; Catskill division of Mather and Hall; and Catskill group of Vanuxem.

According to Hall 1843, and essentially Vanuxem 1842.

Erie division [Devonian]	Chemung, Portage or Nunda (divided into Cashaqua, Gardeau, Portage), Genesee, Tully, Hamilton (divided into Ludlowville, Encrinal, Moscow), and Marcellus.
Helderberg series [Devonian-Silurian]	Corniferous, Onondaga, Schoharie, Cauda-galli, Oriskany, Upper Pentamerus, Encrinal, Delthyris, Pentamerus, Waterlime, Onondaga salt group.
Ontario division [Silurian]	Niagara, Clinton, and Medina.
Champlain division [Silurian-Ordovician-Upper Cambrian]	Oneida or Shawangunk, Grey sandstone, Hudson River group, Utica, Trenton, Black River including Birdseye and Chazy, Calciferous sandrock, and Potsdam.

According to Emmons 1842, Mather 1843, Vanuxem 1842, Hall 1843.

Taconic system [Ordovician and Lower Cambrian]	Granular quartz, Stockbridge limestone, Magnesian slate, and Taconic slate.
Primary or Hypogene system	Metamorphic and Primary rocks.

aran, while the Middle Devonian about Columbus, Ohio, and Louisville, Kentucky, he referred to the same horizon. The Galena-Niagaran error was corrected in 1855, but the Devonian and Mississippian ones remained unadjusted for a long time, and in Iowa until toward the close of the nineteenth century.

Correlations with Europe.—The first effort toward correlating the New York system with those of Europe was made by Conrad in his Notes on American Geology in 1839 (**35**, 243). Here he compares it on faunal grounds with the Silurian system. A more sustained effort was that of Hall in 1843 (**45**, 157), when he said that the Silurian of Murchison was equal to the New York system and embraced the Cambrian, Silurian, and Devonian, which he considered as forming but one system. Hall in 1844 and Conrad earlier were erroneously regarding the Middle Devonian of New York (Hamilton) as "an equivalent of the Ludlow rocks of Mr. Murchison" (**47**, 118, 1844).

In 1846 E. P. De Verneuil spent the summer in America with a view to correlating the formations of the New York system with those of Europe. At this time he had had a wide field experience in France, Germany, and Russia, was president of the Geological Society of France, and "virtually the representative of European geology" (**2**, 153, 1846). Hall says, "No other person could have presented so clear and perfect a coup d'oeil." De Verneuil's results were translated by Hall and with his own comments were published in the Journal in 1848 and 1849 under the title "On the Parallelism of the Paleozoic Deposits of North America with those of Europe." De Verneuil was especially struck with the complete development of American Paleozoic deposits and said it was the best anywhere. On the other hand, he did not agree with the detailed arrangement of the formations in the various divisions of the New York system, and Hall admitted altogether too readily that the terms were proposed "as a matter of concession, and it is to be regretted that such an artificial classification was adopted." De Verneuil's correlations are as follows:

The Lower Silurian system begins with the Potsdam, the analogue of the Obolus sandstone of Russia and

Sweden. The Black River and Trenton hold the position of the Orthoceras limestones of Sweden and Russia, while the Utica and Lorraine are represented by the Graptolite beds of the same countries. Both correlations are in partial error. He unites the Chazy, Birdseye, and Black River in one series, and in another the Trenton, Utica, and Lorraine. Of species common to Europe and America he makes out seventeen.

In the Upper Silurian system, the Oneida and Shawangunk are taken out of the Champlain division, and, with the Medina, are referred to the Silurian, along with all of the Ontario division plus the Lower Helderberg. The Clinton is regarded as highest Caradoc or as holding a stage between that and the Wenlock. The Niagara group is held to be the exact equivalent of the Wenlock, "while the five inferior groups of the Helderberg division represent the rocks of Ludlow." We now know that these Helderberg formations are Lower Devonian in age. De Verneuil unites in one series the Waterlime, Pentamerus, Delthyris, Encrinal, and Upper Pentamerus. Of identical species there are forty common to Europe and America.

The Devonian system De Verneuil begins, "after much hesitation," with the Oriskany and certainly with the five upper members of Hall's Helderberg division, all of the Erie and the Old Red Sandstone. He also adjusts Hall's error by placing in the Devonian the Upper Cliff limestone of Ohio and Indiana, regarded by the former as Silurian. The Oriskany is correlated with the grauwackes of the Rhine, and the Onondaga or Corniferous with the lower Eifelian. Cauda-galli, Schoharie, and Onondaga are united in one series; Marcellus, Hamilton, Tully, and Genesee in another; and Portage and Chemung in a third. Of species common to Europe and America there are thirty-nine.

The Waverly of Ohio and that near Louisville, Kentucky, which Hall had called Chemung, De Verneuil correctly refers to the Carboniferous, but to this Hall does not consent. De Verneuil points out that there are thirty-one species in common between Europe and America. "And as to plants, the immense quantity of terrestrial species identical on the two sides of the Atlantic,

proves that the coal was formed in the neighborhood of lands already emerged, and placed in similar physical conditions."

An analysis of the Paleozoic fossils of Europe and America leads De Verneuil to "the conviction that identical species have lived at the same epoch in America and in Europe, that they have had nearly the same duration, and that they succeeded each other in the same order." This he states is independent of the depth of the seas, and of "the upheavings which have affected the surface of the globe." The species of a period begin and drop out at different levels, and toward the top of a system the whole takes on the character of the next one. "If it happens that in the two countries a certain number of systems, characterized by the same fossils, are superimposed in the same order, whatever may be, otherwise, their thickness and the number of physical groups of which they are composed, it is philosophical to consider these systems as parallel and synchronous."

Because of the dominance of the sandstones and shales in eastern New York, De Verneuil holds that a land lay to the east. The many fucoids and ripple-marks from the Potsdam to the Portage indicated to him shallow water and nearness to a shore.

The Oldest Geologic Eras.—We have seen in previous pages how the Primitive rocks of Arduino and of Werner had been resolved, at least in part, into the systems of the Paleozoic, but there still remained many areas of ancient rocks that could not be adjusted into the accepted scheme. One of the most extensive of these is in Canada, where the really Primitive formations, of granites, gneisses, schists, and even undetermined sediments, abound and are developed on a grander scale than elsewhere, covering more than two million square miles and overlain unconformably by the Paleozoic and later rocks. The first to call attention to them was J. I. Bigsby, a medical staff officer of the British Army, in 1821 (**3**, 254). It was, however, William E. Logan (1798-1875), the "father of Canadian geology," who first unravelled their historical sequence. At first he also called them Primary, but after much work he perceived in them parallel structures and metamorphosed sediments, under-

lain by and associated with pink granites. For the oldest masses, essentially the granites, he proposed the term Laurentian system (1853, 1863) and for the altered and deformed strata, the name Huronian series (1857, 1863). Overlying these unconformably was a third series, the copper-bearing rocks. Since his day a great host of Canadian and American geologists have labored over this, the most intricate of all geology, and now we have the following tentative chronology (Schuchert and Barrell, **38**, 1, 1914):

Late Proterozoic era.
    Keweenawan, Animikian and Huronian periods.
Early Proterozoic era.
    Sudburian period or older Huronian.
Archeozoic era.
    Grenville series, etc.
Cosmic history.

The Taconic System Resurrected.

The Taconic system was first announced by Ebenezer Emmons in 1841, and clearly defined in 1842. It started the most bitter and most protracted discussion in the annals of American geology. After Emmons's subsequent publications had put the Taconic system through three phases, Barrande of Bohemia in 1860-1863 shed a great deal of new and correct light upon it, affirming in a series of letters to Billings that the Taconic fossils are like those of his Primordial system, or what we now call the Middle Cambrian (**31**, 210, 1861, *et seq.*).

In a series of articles published by S. W. Ford in the Journal between 1871 and 1886, there was developed the further new fact that in Rensselaer and Columbia counties, New York, the so-called Hudson River group abounds in "Primordial" fossils wholly unlike those of the Potsdam, and which Ford later on spoke of as belonging to "Lower Potsdam" time.

James D. Dana entered the field of the Taconic area in 1871 and demonstrated that the system also abounds in Ordovician fossiliferous formations. Then came the far-reaching work of Charles D. Walcott, beginning in 1886, which showed that all through eastern New York and into northern Vermont the Hudson River group and

the Taconic system abound not only in Ordovician but also in Cambrian fossils. Finally in 1888 Dana presented a Brief History of Taconic Ideas, and laid away the system with these words (36, 27):

"It is almost fifty years since the Taconic system made its abrupt entrance into geological science. Notwithstanding some good points, it has been through its greater errors, long a hindrance to progress here and abroad . . . But, whether the evil or the good has predominated, we may now hope, while heartily honoring Professor Emmons for his earnest geological labors and his discoveries, that Taconic ideas may be allowed to be and remain part of the past."

As an epitaph Dana placed over the remains of the Taconic system the black-faced numerals **1841-1888.** That the remains of the system, however, and the term Taconic are still alive and demanding a rehearing is apparent to all interested stratigraphers. This is not the place to set the matter right, and all that can be done at the present time is to point out what are the things that still keep alive Emmons's system.

In the typical area of the Taconic system, i. e., in Rensselaer County, Emmons in 1844-1846 produced the fossils *Atops trilineatus* and *Elliptocephala asaphoides*. S. W. Ford, as stated above, later produced from the same general area many other fossils that he demonstrated to be older than the Potsdam sandstone. To this time he gave the name of Lower Potsdam, thus proving on paleontological grounds that at least some part of the Taconic system is older than the New York system, and therefore older than the Hudson River group of Ordovician age.

In 1888 Walcott presented his conclusions in regard to the sequence of the strata in the typical Taconic area and to the north and south of it. He collected Lower Cambrian fossils at more than one hundred localities "within the typical Taconic area," and said that the thickness of his "terrane No. 5" or "Cambrian (Georgia)," now referable to the Lower Cambrian, is "14,000 feet or more." He demonstrated that the Lower Cambrian is infolded with the Lower and Middle Ordovician, and confirmed Emmons's statement that the former rests upon his Primary or Pre-Cambrian masses. Elsewhere, he writes: "To the west of the Taconic range the sec-

tion passes down through the limestone (3) [of Lower and Middle Ordovician age] to the hydromica schists (2) [whose age may also be of early Ordovician], and thence to the great development of slates and shales with their interbedded sparry limestones, calciferous and arenaceous strata, all of which contain more or less of the Olenellus . . . fauna." He then knew thirty-five species in Washington County, New York (35, 401, 1888).

Finally in 1915 Walcott said that in the Cordilleran area of America there was a movement that brought about changes "in the sedimentation and succession of the faunas which serve to draw a boundary line between the Lower and Middle Cambrian series. . . . The length of this period of interruption must have been considerable . . . and when connection with the Pacific was resumed a new fauna that had been developing in the Pacific was then introduced into the Cordilleran sea and constituted the Middle Cambrian fauna. The change in the species from the Lower to the Middle Cambrian fauna is very great." He then goes on to show that in the Appalachian geosyncline there was another movement that shut out the Middle Cambrian *Paradoxides* fauna of the Atlantic realm from this trough, and all deposition as well.

Conclusions.—Accordingly it appears that everywhere in America the Lower Cambrian formations are separated by a land interval of long duration from those of Middle Cambrian time. These formations therefore unite into a natural system of rocks or a period of time. Between Middle and Upper Cambrian time, however, there appears to be a complete transition in the Cordilleran trough, binding these two series of deposits into one natural or diastrophic system. Hence the writer proposes that the Lower Cambrian of America be known as the Taconic system. The Middle and Upper Cambrian series can be continued for the present under the term Cambrian system, a term, however, that is by no means in good standing for these formations, as will be demonstrated under the discussion of the Silurian controversy.

The Silurian Controversy.

Just as in America the base of the Paleozoic was involved in a protracted controversy, so in England the Cambrian-Silurian succession was a subject of long debate between Sedgwick and Murchison, and among the succeeding geologists of Europe. The history of the solution is so well and justly stated in the Journal by James D. Dana under the title "Sedgwick and Murchison: Cambrian and Silurian" (39, 167, 1890), and by Sir Archibald Geikie in his Text-book of Geology, 1903, that all that is here required is to briefly restate it and to bring the solution up to date.

Adam Sedgwick (1785-1873) and R. I. Murchison (1792-1871) each began to work in the areas of Cambria (Wales) and Siluria (England) in 1831, but the terms Cambrian and Silurian were not published until 1835. Murchison was the first to satisfactorily work out the sequence of the Silurian system because of the simpler structural and more fossiliferous condition of his area. Sedgwick, on the other hand, had his academic duties to perform at Cambridge University, and being an older and more conservative man, delayed publishing his final results, because of the further fact that his area was far more deformed and less fossiliferous. In 1834 they were working in concert in the Silurian area, and Sedgwick said: "I was so struck by the clearness of the natural sections and the perfection of his workmanship that I received, I might say, with implicit faith everything which he then taught me. . . . The whole 'Silurian system' was by its author placed *above* the great undulating slate-rocks of South Wales." At that time Murchison told Sedgwick that the Bala group of the latter, now known to be in the middle of the Lower Silurian, could not be brought within the limits of the Silurian system, and added, "I believe it to plunge under the true Llandeilo-flags," now placed next below the Bala and above the Arenig, which at the present is regarded as at the base of the Ordovician.

The Silurian system was defined in print by Murchison in July, 1835, the Upper Silurian embracing the Ludlow and Wenlock, while the Lower Silurian was based on the

Caradoc and Llandeilo. Murchison's monumental work, The Silurian System, of 100 pages and many plates of fossils, appeared in 1838.

The Cambrian system was described for the first time by Sedgwick in August, 1835, but the completed work—a classic in geology—Synopsis of the Classification of the British Palæozoic Rocks, along with M'Coy's Descriptions of British Palæozoic Fossils, did not appear until 1852-1855. Sedgwick's original Upper Cambrian included the greater part of the chain of the Berwyns, where he said it was connected with the Llandeilo flags of the Silurian. The Middle Cambrian comprised the higher mountains of Cærnarvonshire and Merionethshire, and the Lower Cambrian was said to occupy the southwest coast of Cærnarvonshire, and to consist of chlorite and mica schists, and some serpentine and granular limestone. In 1853 it was seen that the fossiliferous Upper Cambrian included the Arenig, Llandeilo, Bala, Caradoc, Coniston, Hirnant, and Lower Llandovery. On the other hand, it was not until long after Murchison and Sedgwick passed away that the Middle and Lower Cambrian were shown to have fossils, but few of those that characterize what is now called Lower, Middle, and Upper Cambrian time.

Not until long after the original announcement of the Cambrian system did Sedgwick become aware "of the unfortunate mischief-involving fact" that the most fossiliferous portion of the Cambrian—the Upper Cambrian—and at that time the only part yielding determinable fossils, when compared with the Lower Silurian was seen to be an equivalent formation but with very different lithologic conditions. He began to see in 1842 that his Cambrian was in conflict with the Silurian system, and four years later there were serious divergencies of views between himself and Murchison. The climax of the controversy was attained in 1852, when Sedgwick was extending his Cambrian system upwards to include the Bala, Llandeilo, and Caradoc, a proceeding not unlike that of Murchison, who earlier had been extending his Silurian downward through all of the fossiliferous Cambrian to the base of the Lingula flags.

Dana in his review of the Silurian-Cambrian controversy states: "The claim of a worker to affix a name to a

series of rocks first studied and defined by him cannot be disputed." We have seen that Murchison had priority of publication in his term Silurian over Sedgwick's Cambrian, but that in a complete presentation, both stratigraphically and faunally, the former had years of prior definition. What has even more weight is that geologists nearly everywhere had accepted Murchison's Silurian system as founded upon the Lower and Upper Silurian formations. A nomenclature once widely accepted is almost impossible to dislodge. However, in regard to the controversy it should not be forgotten that it was only Murchison's *Lower* Silurian that was in conflict with Sedgwick's *Upper* Cambrian. As for the rest of the Cambrian, that was not involved in the controversy.

Dana goes on to state that science may accept a name, or not, according as it is, or is not, needed. In the progress of geology, he thought that the time had finally been reached when the name Cambrian was a necessity, and he included both Cambrian and Silurian in the geological record. The "Silurian," however, included the Lower and Upper Silurian—not one system of rocks, but two.

It is now twenty-seven years since Dana came to this conclusion, at a time when it was believed that there was more or less continuous deposition not only between the formations of a system but between the systems themselves as well. To-day many geologists hold that in the course of time the oceans pulsate back and forth over the continents, and accordingly that the sequence of marine sedimentation in most places must be much broken, and to-day we know that the breaks or land intervals in the marine record are most marked between the eras, and shorter between all or at least most of the periods. Furthermore, in North America, we have learned that the breaks between the systems are most marked in the interior of the continent and less so on or toward its margins.

Hardly any one now questions the fact of a long land interval between the Lower Silurian and Upper Silurian in England, and it is to Sedgwick's credit that he was the first to point out this fact and also the presence of an unconformity. It therefore follows that we cannot continue to use Silurian system in the sense proposed by

Murchison, since it includes two distinct systems or periods. Dana, in the last edition of his Manual of Geology (1895), also recognizes two systems, but curiously he saw nothing incongruous in calling them "Lower Silurian era" and "Upper Silurian era." It certainly is not conducive to clear thinking, however, to refer to two systems by the one name of Silurian and to speak of them individually as Lower and Upper Silurian, thus giving the impression that the two systems are but parts of one—the Silurian. Each one of the parts has its independent faunal and physical characters.

We must digress a little here and note the work of Joachim Barrande (1799-1883) in Bohemia. In 1846 he published a short account of the "Silurian system" of Bohemia, dividing it into étages lettered C to H. Between 1852 and 1883 he issued his "Système Silurien du Centre de la Bohême," in eighteen quarto volumes with 5568 pages of text and 798 plates of fossils—a monumental work unrivalled in paleontology. In the first volume the geology of Bohemia is set forth, and here we see that étages A and B are Azoic or pre-Cambrian, and C to H make up his Silurian system. Etage C has his "Primordial fauna," now known to be of Paradoxides or Middle Cambrian time, while D is Lower Silurian, E is Upper Silurian, F is Lower Devonian, and G and H are Middle Devonian. From this it appears that Barrande's Silurian system is far more extensive than that of Murchison, embracing twice as many periods as that of England and Wales.

About 1879 there was in England a nearly general agreement that Cambrian should embrace Barrande's Primordial or Paradoxides faunas, and in the North Wales area be continued up to the top of the Tremadoc slates. To-day we would include Middle and Upper Cambrian. Lower Cambrian in the sense of containing the Olenellus faunas was then unknown in Great Britain.

Lapworth, recognizing the distinctness of the Lower Silurian as a system, proposed in 1879 to recognize it as such, and named it Ordovician, restricting Silurian to Murchison's Upper Silurian. This term has not been widely used either in Great Britain or on the Continent, but in the last twenty years has been accepted more and

more widely in America. Even here, however, it is in direct conflict with the term Champlain, proposed by the New York State Geologist in 1842.

In 1897 the International Geological Congress published E. Renevier's Chronographie Géologique, wherein we find the following:

Silurian Period.	Upper or Silurian (Murchison, restricted, 1835).	Ludlowian (Murchison 1839). Wenlockian (Murchison 1839). Landoverian (Murchison).
	Middle or Ordovician (Lapworth 1879).	Caradocian (Murchison 1839). Landeilian (Murchison 1839). Arenigian (Sedgwick 1847).
	Lower or Cambrian (Sedgwick, restricted, 1835).	Potsdamian (Emmons 1838). Menevian (Salter and Hicks 1865). Georgian (Hitchcock 1861).

Regarding this period, which, by the way, is not very unlike that of Barrande, Renevier remarks that it is "as important as the Cretaceous or the Jurassic. Lapworth even gives it a value of the first order equal to the Protozoic era."

In the above there is an obvious objection in the double usage of the term Silurian, and this difficulty was met later on in Lapparent's Traité by the proposal to substitute Gothlandian for Silurian. Of this change Geikie remarks: "Such an arrangement . . . might be adopted if it did not involve so serious an alteration of the nomenclature in general use." On the other hand, if diastrophism and breaks in the stratigraphic and faunal sequence are to be the basis for geologic time divisions, we cannot accept the above scheme, for it recognizes but one period where there are at least four in nature.

Conclusions.—We have arrived at a time when our knowledge of the stratigraphic and faunal sequence, plus the orogenic record as recognized in the principle of diastrophism, should be reflected in the terminology of the geologic time-table. It would be easy to offer a satisfactory nomenclature if we were not bound by the law of priority in publication, and if no one had the geologic chronology of his own time ingrained in his memory. In addition, the endless literature, with its accepted nomenclature, bars our way. Therefore with a view of

creating the least change in geologic nomenclature, and of doing the greatest justice to our predecessors that the present conditions of our knowledge will allow, the following scheme is offered:

Silurian period. Llandovery to top of Ludlow in Europe. Alexandrian-Cataract-Medina to top of Manlius in America.

Champlain (1842) or Ordovician (1879) period. Arenig to top of Caradoc in Europe. Beekmantown to top of Richmondian in America.

Cambrian period. In the Atlantic realm, begins with the Paradoxides, and in the Pacific, with the Bathyuriscus and Ogygopsis faunas. The close is involved in Ulrich's provisionally defined Ozarkian system. When the latter is established, the Ozarkian period will hold the time between the Ordovician and the Cambrian.

Taconic period. For the world-wide Olenellus or Mesonacidæ faunas.

Paleogeography.

When geologists began to perceive the vast significance of Hutton's doctrine that "the ruins of an earlier world lie beneath the secondary strata," and that great masses of bedded rocks are separated from one another by periods of mountain making and by erosion intervals, it was natural for them to look for the lands that had furnished the débris of the accumulated sediments. In this way paleogeography had its origin, but it was at first of a descriptive and not of a cartographic nature.

The word paleogeography was proposed by T. Sterry Hunt in 1872 in a paper entitled "The Paleogeography of the North American Continent," and published in the Journal of the American Geographical Society for that year. It has to do, he says, with the "geographical history of these ancient geological periods." It was again prominently used by Robert Etheridge in his presidential address before the Geological Society of London in 1881. Since Canu's use of the term in 1896, it has been frequently seen in print, and now is generally adopted to signify the geography of geologic time.

The French were the first to make paleogeographic maps, and Jules Marcou relates in 1866 that Elie de Beaumont, as early as March, 1831, in his course in the College of France and at the Paris School of Mines, used

to outline the relation of the lands and the seas in the center of Europe at the different great geologic periods. His first printed paleogeographic map appeared in 1833, and was of early Tertiary time. Other maps by Beaumont were published by Beudant in 1841-1842. The Sicilian geologist Gemmellaro published six maps of his country in 1834, and the Englishman De La Beche had one in the same year. In America the first to show such maps was Arnold Guyot in his Lowell lectures of 1848. James D. Dana published three in the 1863 edition of his Manual of Geology. Of world paleogeographic maps, Jules Marcou produced the first of Jurassic time, publishing it in France in 1866, but the most celebrated of these early attempts was the one by Neumayr published in 1883 in connection with his Ueber klimatische Zonen während der Jura- und Kreidezeit.

The first geologist to produce a series of maps showing the progressive geologic geography of a given area was Jukes-Brown, who in the volume entitled "The Building of the British Isles," 1888, included fifteen such maps. Karpinsky published fourteen maps of Russia, and in 1896 Canu in his Essai de paléogéographie has fifty-seven of France and Belgium. Lapparent's Traité of 1906 is famous for paleogeographic maps, for he has twenty-three of the world, thirty-four of Europe, twenty-five of France, and ten taken from other authors. Schuchert in 1910 published fifty-two to illustrate the paleogeography of North America, and also gave an extended list of such published maps. Another article on the subject is by Th. Arldt, "Zur Geschichte der Paläogeographischen Rekonstructionen," published in 1914. Edgar Dacqué in 1913 also produced a list in his Paläogeographischen Karten, and two years later appeared his book of 500 pages, Grundlagen und Methoden der Paläogeographie, where the entire subject is taken up in detail.

Conclusions.—Since 1833 there have been published not less than 500 different paleogeographic maps, and of this number about 210 relate to North America. Nevertheless paleogeography is still in its infancy, and most maps embrace too much geologic time, all of them tens of thousands, and some of them millions of years. The geographic maps of the present show the conditions of

the strand-lines of to-day, and those made fifty years ago have to be revised again and again if they are to be of value to the mariner and merchant. Therefore in our future paleogeographic maps the tendency must ever be toward smaller amounts of geologic time, if we are to show the actual relation of water to land and the movements of the periodic floodings. Moreover, the ancient shore lines are all more or less hypothetic and are drawn in straight or sweeping curves, unlike modern strands with their bays, deltas, and headlands, and the ancient lands are featureless plains. We must also pay more attention to the distribution of brackish- and fresh-water deposits. The periodically rising mountains will be the first topographic features to be shown upon the ancient lands, and then more and more of the drainage and the general climatic conditions must be portrayed. In the seas, depth, temperature, and currents are yet to be deciphered. Finally, other base maps than those of the geography of to-day will have to be made, allowing for the compression of the mountainous areas, if we are to show the true geographic configurations of the lands and seas of any given geologic time.

Paleometeorology.

In accordance with the Laplacian theory, announced at the beginning of the nineteenth century, all of the older geologists held that the earth began as a hot star, and that in the course of time it slowly cooled and finally attained its present zonal cold to tropical climatic conditions. That the earth had very recently passed through a much colder climate, a glacial one, came into general acceptance only during the latter half of the previous century.

Rise.—Our knowledge of glacial climates had its origin in the Alps, that wonderland of mountains and glaciers. The rise of this knowledge in the Alps is told in a charming and detailed manner by that erratic French-American geologist, Jules Marcou (1824-1898), in his Life, Letters, and Works of Louis Agassiz, 1896. He relates that the Alpine chamois hunter Perraudin in 1815 directed the attention of the engineer De Charpentier to the fact "that the large boulders perched on the sides of

the Alpine valleys were carried and left there by glaciers." For a long time the latter thought the conclusion extravagant, and in the meantime Perraudin told the same thing to another engineer, Venetz. He, in 1829, convinced of the correctness of the chamois hunter's views, presented the matter before the Swiss naturalists then meeting at St. Bernard's. Venetz "told the Society that his observations led him to believe that the whole Valais has been formerly covered by an immense glacier and that it even extended outside of the canton, covering all the Canton de Vaud, as far as the Jura Mountains, carrying the boulders and erratic materials, which are now scattered all over the large Swiss valley." Eight years earlier, in 1821, similar views had been presented by the same modest naturalist before the Helvetic Society, but it was not until 1833 that De Charpentier found the manuscript and had it published. Venetz's conclusions were that all of the glaciers of the Bagnes valley "have very recognizable moraines, which are about a league from the present ice." "The moraines . . . date from an epoch which is lost in the night of time." Then in 1834 De Charpentier read a paper before the same society, meeting at Lucerne. "Seldom, if ever, has such a small memoir so deeply excited the scientific world. It was received at first with incredulity and even scorn and mockery, Agassiz being among its opponents." The paper was published in 1835, first at Paris, then at Geneva, and finally in Germany. It "attracted much attention, and the smile of incredulity with which it was received when read at Lucerne soon changed into a desire to know more about it."

Louis Agassiz (1807-1873), who had long been acquainted with his countryman, De Charpentier, spent several months with him in 1836, and together they studied the glaciers of the Alps. Agassiz was at first "adverse to the hypothesis, and did not believe in the great extension of glaciers and their transportation of boulders, but on the contrary, was a partisan of Lyell's theory of transport by icebergs and ice-cakes . . . but from being an adversary of the glacial theory, he returned to Neuchâtel an enthusiastic convert to the views of Venetz and De Charpentier. . . . With his

power of quick perception, his unmatched memory, his perspicacity and acuteness, his way of classifying, judging and marshalling facts, Agassiz promptly learned the whole mass of irresistible arguments collected patiently during seven years by De Charpentier and Venetz, and with his insatiable appetite and that faculty of assimilation which he possessed in such a wonderful degree, he digested the whole doctrine of the glaciers in a few weeks."

In July, 1837, Agassiz presented as his presidential address before the Helvetic Society his memorable "Discours de Neuchâtel," which was "the starting point of all that has been written on the Ice-age,"—a term coined at the time by his friend Schimper, a botanist. The first part of this address is reprinted in French in Marcou's book on Agassiz. The address was received with astonishment, much incredulity, and indifference. Among the listeners was the great German geologist Von Buch, who "was horrified, and with his hands raised towards the sky, and his head bowed to the distant Bernese Alps, exclaimed: 'O Sancte de Saussure, ora pro nobis!'" Even De Charpentier "was not gratified to see his glacial theory mixed with rather uncalled for biological problems, the connection of which with the glacial age was more than problematic." Agassiz was then a Cuvierian catastrophist and creationist, and advanced the idea of a series of glacial ages to explain the destruction of the geologic succession of faunas! Curiously, this theory was at once accepted by the American paleontologist T. A. Conrad (**35**, 239, 1839).

The classics in glacial geology are Agassiz's Etudes sur les Glaciers, 1840, and De Charpentier's Essai sur les Glaciers, 1841. Of the latter book, Marcou states that it has been said: "It is impossible to be truly a geologist without having read and studied it." In the English language there is Tyndall's Glaciers of the Alps, 1860.

The progress of the ideas in regard to Pleistocene glaciation is presented in the following chapter by H. E. Gregory.

Older Glacial Climates.—Hardly had the Pleistocene glacial climate been proved, when geologists began to point out the possibility of even earlier ones. An enthu-

siastic Scotch writer, Sir Andrew Ramsay, in 1855 described certain late Paleozoic conglomerates of middle England, which he said were of glacial origin, but his evidence, though never completely gainsaid, has not been generally accepted. In the following year, an Englishman, Doctor W. T. Blanford, said that the Talchir conglomerates of central and southern India were of glacial origin, and since then the evidence for a Permian glacial climate has been steadily accumulating. Africa is the land of tillites, and here in 1870 Sutherland pointed out that the conglomerates of the Karroo formation were of glacial origin. Australia also has Permian glacial deposits, and they are known widely in eastern Brazil, the Falkland Islands, the vicinity of Boston, and elsewhere. So convincing is this testimony that all geologists are now ready to accept the conclusion that a glacial climate was as wide-spread in early Permian time as was that of the Pleistocene.[1]

In South Africa, beneath the marine Lower Devonian, occurs the Table Mountain series, 5000 feet thick. The series is essentially one of quartzites, with zones of shales or slates and with striated pebbles up to 15 inches long. The latter occur in pockets and seem to be of glacial origin. There are here no typical tillites, and no striated undergrounds have so far been found. While the evidence of the deposits appears to favor the conclusion that the Table Mountain strata were laid down in cold waters with floating ice derived from glaciers, it is as yet impossible to assign these sediments a definite geologic age. They are certainly not younger than the Lower Devonian, but it has not yet been established to what period of the early Paleozoic they belong.

In southeastern Australia occur tillites of wide distribution that lie conformably beneath, but sharply separated from the fossiliferous marine Lower Cambrian strata. David (1907), Howchin (1908), and other Australian geologists think they are of Cambrian time, but to the writer they seem more probably late Proterozoic in age. In arctic Norway Reusch discovered unmistakable tillites in 1891, and this occurrence was confirmed by Strahan in 1897. It is not yet certainly known what their age is, but it appears to be late Proterozoic rather

than early Paleozoic. Other undated Proterozoic tillites occur in China (Willis and Blackwelder 1907), Africa (Schwarz 1906), India (Vredenburg 1907), Canada (Coleman 1908), and possibly in Scotland.

The oldest known tillites are described by Coleman in 1907, and occur at the base of the Lower Huronian or in early Proterozoic time. They extend across northern Ontario for 1000 miles, and from the north shore of Lake Huron northward for 750 miles.

Fossils as Climatic Indexes.—Paleontologists have long been aware that variations in the climates of the past are indicated by the fossils, and Neumayr in 1883 brought the evidence together in his study of climatic zones mentioned elsewhere. Plants, and corals, cephalopods, and foraminifers among marine animals, have long been recognized as particularly good "life thermometers." In fact, all fossils are climatic indicators to some extent, and a good deal of evidence concerning paleometeorology has been discerned in them. This evidence is briefly stated in the paper by Schuchert already alluded to, and in W. D. Matthew's Climate and Evolution, 1915.

Sediments as Climatic Indexes.—Johannes Walther in the third part of his Einleitung—Lithogenesis der Gegenwart, 1894—is the first one to decidedly direct attention to the fact that the sediments also have within themselves a climatic record. In America Joseph Barrell has since 1907 written much on the same subject. On the other hand, the periodic floodings of the continents by the oceans, and the making of mountains, due to the periodic shrinkage of the earth, as expressed in T. C. Chamberlin's principle of diastrophism and in his publications since 1897, are other criteria for estimating the climates of the past.

Conclusions.—In summation of this subject Schuchert says:

"The marine 'life thermometer' indicates vast stretches of time of mild to warm and equable temperatures, with but slight zonal differences between the equator and the poles. The great bulk of marine fossils are those of the shallow seas, and the evolutionary changes recorded in these 'medals of creation' are slight throughout vast lengths of time that are punctuated by

short but decisive periods of cooled waters and great mortality, followed by quick evolution, and the rise of new stocks. The times of less warmth are the *miotherm* and those of greater heat the *pliotherm* periods of Ramsay.

On the land the story of the climatic changes is different, but in general the equability of the temperature simulates that of the oceanic areas. In other words, the lands also had long-enduring times of mild to warm climates. Into the problem of land climates, however, enter other factors that are absent in the oceanic regions, and these have great influence upon the climates of the continents. Most important of these is the periodic warm-water inundation of the continents by the oceans, causing insular climates that are milder and moister. With the vanishing of the floods somewhat cooler and certainly drier climates are produced. The effects of these periodic floods must not be underestimated, for the North American continent was variably submerged at least seventeen times, and over an area of from 154,000 to 4,000,000 square miles.

When to these factors is added the effect upon the climate caused by the periodic rising of mountain chains, it is at once apparent that the lands must have had constantly varying climates. In general the temperature fluctuations seem to have been slight, but geographically the climates varied between mild to warm pluvial, and mild to cool arid. The arid factor has been of the greatest import to the organic world of the lands. Further, when to all of these causes is added the fact that during emergent periods the formerly isolated lands were connected by land bridges, permitting intermigration of the land floras and faunas, with the introduction of their parasites and parasitic diseases, we learn that while the climatic environment is of fundamental importance it is not the only cause for the more rapid evolution of terrestrial life . . .

Briefly, then, we may conclude that the markedly varying climates of the past seem to be due primarily to periodic changes in the topographic form of the earth's surface, plus variations in the amount of heat stored by the oceans. The causation for the warmer interglacial climates is the most difficult of all to explain, and it is here that factors other than those mentioned may enter.

Granting all this, there still seems to lie back of all these theories a greater question connected with the major changes in paleometeorology. This is: What is it that forces the earth's topography to change with varying intensity at irregularly rhythmic intervals? . . . Are we not forced to conclude that the earth's shape changes periodically in response to gravitative forces that alter the body-form?"

Evolution.

Modern evolution, or the theory of life continuously descending from life with change, may be said to have had its first marked development in Comte de Buffon (1707-1788), a man of wealth and station, yet an industrious compiler, a brilliant writer, and a popularizer of science. He was not, however, a true scientific investigator, and his monument to fame is his Histoire Naturelle, in forty-four volumes, 1749-1804. A. S. Packard in his book on Lamarck, his Life and Work, 1901, concludes in regard to Buffon as follows:

"The impression left on the mind, after reading Buffon, is that even if he threw out these suggestions and then retracted them, from fear of annoyance or even persecution from the bigots of his time, he did not himself always take them seriously, but rather jotted them down as passing thoughts . . . They appeared thirty-four years before Lamarck's theory, and though not epoch-making, they are such as will render the name of Buffon memorable for all time."

Chevalier de Lamarck (1744-1829) may justly be regarded as the founder of the doctrine of modern evolution. Previous to 1794 he was a believer in the fixity of species, but by 1800 he stood definitely in favor of evolution. Locy in his Biology and its Makers, 1908, states his theories in the following simplified form:

"Variations of organs, according to Lamarck, arise in animals mainly through use and disuse, and new organs have their origin in a physiological need. A new need felt by the animal [due to new conditions in its life, or the environment] expresses itself on the organism, stimulating growth and adaptations in a particular direction."

To Lamarck, "inheritance was a simple, direct transmission of those superficial changes that arise in organs within the lifetime of an individual owing to use and disuse." This part of his theory has come to be known as "the inheritance of acquired characters."

Georges Cuvier (1769-1832), a peer of France, was a decided believer in the fixity of species and in their creation through divine acts. In 1796 he began to see that among the fossils so plentiful about Paris many were of

extinct forms, and later on that there was a succession of wholly extinct faunas. This at first puzzling phenomenon he finally came to explain by assuming that the earth had gone through a series of catastrophes, of which the Deluge was the most recent but possibly not the last. With each catastrophe all life was blotted out, and a new though improved set of organisms was created by divine acts. The Cuvierian theory of catastrophism was widely accepted during the first half of the nineteenth century, and in America Louis Agassiz was long its greatest exponent. It was this theory and the dominance of the brilliant Cuvier, not only in science but socially as well, that blotted out the far more correct views of the more philosophical Lamarck, who held that life throughout the ages had been continuous and that through individual effort and the inheritance of acquired characters had evolved the wonderful diversity of the present living world.

In 1830 there was a public debate at Paris between Cuvier and Geoffroy Saint-Hilaire, the one holding to the views of the fixity of species and creation, the other that life is continuous and evolves into better adapted forms. Cuvier, a gifted speaker and the greatest debater zoology ever had, with an extraordinary memory that never failed him, defeated Saint-Hilaire in each day's debate, although the latter was in the right.

A book that did a great deal to prepare the English-speaking people for the coming of evolution was "Vestiges of Creation," published in 1844 by an unknown author. In Darwin's opinion, "the work, from its powerful and brilliant style . . . has done excellent service . . . in thus preparing the ground for the reception of analogous views." This book was recommended to the readers of the Journal (**48**, 395, 1845) with the editorial remark that "we cannot subscribe to all of the author's views."

We can probably best illustrate the opinions of Americans on the question of evolution just before the appearance of Darwin's great work by directing attention to James D. Dana's Thoughts on Species (**24**, 305, 1857). After reading this article and others of a similar nature by Agassiz, one comes to the opinion that unconsciously

both men are proving evolution, but consciously they are firm creationists. It is astonishing that with their extended and minute knowledge of living organisms and their philosophic type of mind neither could see the true significance of the imperceptible transitions between some species, which if they do not actually pass into, at least shade towards, one another.

Dana speaks of "the endless diversities in individuals" that compose a species, and then states that a living species, like an inorganic one, "is based on a specific amount or condition of concentered force defined in the act or law of creation." Species, he says, are permanent, and hybrids "cannot seriously trifle with the true units of nature, and at the best, can only make temporary variations." "We have therefore reason to believe from man's fertile intermixture, that he is one in species: and that all organic species are divine appointments which cannot be obliterated, unless by annihilating the individuals representing the species."

Through the activities of the French the world was prepared for the reception of evolution, and now it was already in the minds of many advanced thinkers. In 1860 Asa Gray sent to the editor of the Journal (**29**, 1) an article by the English botanist, Joseph D. Hooker, entitled "On the Origination and Distribution of Species," with these significant remarks:

"The essay cannot fail to attract the immediate and profound attention of scientific men . . . It has for some time been manifest that a re-statement of the Lamarckian hypothesis is at hand. We have this, in an improved and truly scientific form, in the theories which, recently propounded by Mr. Darwin, followed by Mr. Wallace, are here so ably and altogether independently maintained. When these views are fully laid before them, the naturalists of this country will be able to take part in the interesting discussion which they will not fail to call forth."

Hooker took up a study of the flora of Tasmania, of which the above cited article is but a chapter, with a view to trying out Darwin's theory, and he now accepts it. He says, "Species are derivative and mutable." "The limits of the majority of species are so undefinable that few naturalists are agreed upon them."

Asa Gray had received from Darwin an advance copy of the book that was to revolutionize the thought of the world, and at once wrote for the Journal a Review of Darwin's Theory on the Origin of Species by means of Natural Selection (29, 153, 1860). This is a splendid, critical but just, scientific review of Darwin's epoch-making book. Evidently views similar to those of the English scientist had long been in the mind of Gray, for he easily and quickly mastered the work. He is easy on Dana's Thoughts on Species, which were idealistic and not in harmony with the naturalistic views of Darwin. On the other hand, he contrasts Darwin's views at length with those of the creationists as exemplified by Louis Agassiz, and says "The widest divergence appears."

Gray says in part:

"The gist of Mr. Darwin's work is to show that such varieties are gradually diverged into species and genera through natural selection; that natural selection is the inevitable result of the struggle for existence which all living things are engaged in; and that this struggle is an unavoidable consequence of several natural causes, but mainly of the high rate at which all organic beings tend to increase.

Darwin is confident that intermediate forms must have existed; that in the olden times when the genera, the families and the orders diverged from their parent stocks, gradations existed as fine as those which now connect closely related species with varieties. But they have passed and left no sign. The geological record, even if all displayed to view, is a book from which not only many pages, but even whole alternate chapters have been lost out, or rather which were never printed from the autographs of nature. The record was actually made in fossil lithography only at certain times and under certain conditions (i. e., at periods of slow subsidence and places of abundant sediment); and of these records all but the last volume is out of print; and of its pages only local glimpses have been obtained. Geologists, except Lyell, will object to this,—some of them moderately, others with vehemence. Mr. Darwin himself admits, with a candor rarely displayed on such occasions, that he should have expected more geological evidence of transition than he finds, and that all the most eminent paleontologists maintain the immutability of species.

The general fact, however, that the fossil fauna of each period as a whole is nearly intermediate in character between the preceding and the succeeding faunas, is much relied on. We

are brought one step nearer to the desired inference by the similar 'fact,' insisted on by all paleontologists, that fossils from two consecutive formations are far more closely related to each other, than are the fossils of two remote formations.

It is well said that all organic beings have been formed on two great laws; Unity of type, and Adaptation to the conditions of existence . . . Mr. Darwin harmonizes and explains them naturally. Adaptation to the conditions of existence is the result of Natural Selection; Unity of type, of unity of descent."

Gray's article was soon followed by another one from Agassiz on Individuality and Specific Differences among Acalephs, but the running title is "Prof. Agassiz on the Origin of Species" (**30**, 142, 1860). Agassiz stoutly maintains his well known views, and concludes as follows:

"Were the transmutation theory true, the geological record should exhibit an uninterrupted succession of types blending gradually into one another. The fact is that throughout all geological times each period is characterized by definite specific types, belonging to definite genera, and these to definite families, referable to definite orders, constituting definite classes and definite branches, built upon definite plans. Until the facts of Nature are shown to have been mistaken by those who have collected them, and that they have a different meaning from that now generally assigned to them, I shall therefore consider the transmutation theory as a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency."

Dana, in reviewing Huxley's well known book, Man's Place in Nature (**35**, 451, 1863), holds that man is apart from brute nature because man exhibits "extreme cephalization" in that he has arms that no longer are used in locomotion but go rather with the head, and because he has a far higher mentality and speech. As for the Darwinian theory, the evidence, he says, "comes from lower departments of life, and is acknowledged by its advocates to be exceedingly scanty and imperfect."

The growth of evolution is set forth in the Journal in Asa Gray's article on Charles Darwin (**24**, 453, 1882), which speaks of the latter as "the most celebrated man of science of the nineteenth century," and, in addition, as "one of the most kindly and charming, unaffected, simple-hearted, and lovable of men." In regard to the rise

of evolution in America, more can be had from Dana's paper on Asa Gray (35, 181, 1888). Here we read, as a sequel to his Thoughts on Species, that the "paper may be taken, perhaps, as a culmination of the past, just as the new future was to make its appearance." Finally, in this connection there should be mentioned O. C. Marsh's paper on Thomas Henry Huxley (50, 177, 1895), wherein is recorded the latter's share in the upbuilding of the evolutionary theory.

We have seen that originally Dana was a creationist, but in the course of his long and fruitful life he gradually became an evolutionist, and rather a Neo-Lamarckian than a Darwinian. This change may be traced in the various editions of his Manual of Geology, and in the last edition of 1895 he says his "speculative conclusions" of 1852 in regard to the origin of species are not "in accord with the author's present judgment." "The evidence in favor of evolution by variation is now regarded as essentially complete." On the other hand, while man is "unquestionably" closely related in structure to the man-apes, yet he is not linked to them but stands apart, through "the intervention of a Power above Nature. . . . Believing that Nature exists through the will and ever-acting power of the Divine Being, and . . . that the whole Universe is not merely dependent on, but actually is, the Will of one Supreme Intelligence, Nature, with Man as its culminant species, is no longer a mystery."

In America most of the paleontologists are Neo-Lamarckian, a school that was developed independently by E. D. Cope (1840-1897) through the vertebrate evidence, and by Alpheus Hyatt (1838-1902) mainly on the evidence of the ammonites. They hold that variations and acquired characters arise through the effects of the environment, the mechanics of the organism resulting from the use and disuse of organs, etc. One of the leading exponents of this school is A. S. Packard, whose book on Lamarck, His Life and Work, 1901, fully explains the doctrines of the Neo-Lamarckians.

The Growth of Invertebrate Paleontology.

How and by whom paleontology has been developed has been fully stated in the Journal in a very clear man-

ner by Professor Marsh in his memorable presidential address of 1879, History and Methods of Palæontological Discovery (**18**, 323, 1879), and by Karl von Zittel in his most interesting book, History of Geology and Palæontology, 1901. In this discussion we shall largely follow Marsh.

The science of paleontology has passed through four periods, the first of them the long *Mystic period* extending up to the beginning of the seventeenth century, when the idea that fossils were once living things was only rarely perceived. The second period was the *Diluvial period* of the eighteenth century, when nearly everyone regarded the fossils as remains of the Noachian deluge. With the beginnings of the nineteenth century there arose in western Europe the knowledge that fossils are the "medals of creation" and that they have a chronogenetic significance; also that life had been periodically destroyed through world-wide convulsions in nature. From about 1800 to 1860 was the time of the creationists and catastrophists, which may be known as the *Catastrophic period.* The fourth period began in 1860 with Darwin's Origin of Species. Since that time the theory of evolution has pervaded all work in paleontology, and accordingly this time may be known as the *Evolutionary period.*

Mystic Period.—The Mystic period in paleontology begins with the Greeks, five centuries before the present era, and continues down to the beginning of the seventeenth century of our time. Some correctly saw that the fossils were once living marine animals, and that the sea had been where they now occur. Others interpreted fossil mammal bones as those of human giants, the Titans, but the Aristotelian view that they were of spontaneous generation through the hidden forces of the earth dominated all thought for about twenty centuries.

In the sixteenth century canals were being dug in Northern Italy, and the many fossils so revealed led to a fierce discussion as to their actual nature. Leonardo da Vinci (1452-1519) opposed the commonly accepted view of their spontaneous generation and said that they were the remains of once living animals and that the sea had been where they occur. "You tell me," he said, "that

Nature and the influence of the stars have formed these shells in the mountains; then show me a place in the mountains where the stars at the present day make shelly forms of different ages, and of different species in the same place." However, nothing came of his teachings and those of his countryman Fracastorio (1483-1553), who further ridiculed the idea that they were the remains of the deluge. The first mineralogist, Agricola, described them as minerals—fossilia—and said that they arose in the ground from fatty matter set in fermentation by heat. Others said that they were freaks of nature. Martin Lister (1638-1711) figured fossils side by side with living shells to show that they were extinct forms of life. In the seventeenth century, and especially in Italy and Germany, many books were published on fossils, some with illustrations so accurate that the species can be recognized to-day. Finally, toward the close of this century the influence of Aristotle and the scholastic tendency to disputation came more or less to an end. Fossils were already to many naturalists once living plants and animals. Marsh states: "The many collections of fossils that had been brought together, and the illustrated works that had been published about them, were a foundation for greater progress, and, with the eighteenth century, the second period in the history of paleontology began."

Diluvial Period.—During the eighteenth century many more books on fossils were published in western Europe, and now the prevalent explanation was that they were the remains of the Noachian deluge. For nearly a century theologians and laymen alike took this view, and some of the books have become famous on this account, but the diluvial views sensibly declined with the close of the eighteenth century.

The true nature of fossils had now been clearly determined. They were the remains of plants and animals, deposited long before the deluge, part in fresh water and part in the sea. "Some indicated a mild climate, and some the tropics. That any of these were extinct species, was as yet only suspected." Yet before the close of the century there were men in England and France who pointed out that different formations had different fossils and

that some of them were extinct. These views then led to many fantastic theories as to how the earth was formed—dreams, most of them have been called. Marsh says:

> "The dominant idea of the first sixteen centuries of the present era was, that the universe was made for Man. This was the great obstacle to the correct determination of the position of the earth in the universe, and, later, of the age of the earth. . . . In a superstitious age, when every natural event is referred to a supernatural cause, science cannot live . . . Scarcely less fatal to the growth of science is the age of Authority, as the past proves too well. With freedom of thought, came definite knowledge, and certain progress;—but two thousand years was long to wait."

One of the most significant publications of this period was Linnæus's Systema Naturæ, which appeared in 1735. In this work was introduced binomial nomenclature, or the system of giving each plant and animal species a generic and specific name, as *Felis leo* for the lion. The system was, however, not established until the tenth edition of the work in 1758, which became the starting point of zoological nomenclature. Since then there has been added another canon, the law of priority, which holds that the first name applied to a given form shall stand against all later names given to the same organism.

Catastrophic Period.—With the beginning of the nineteenth century there started a new era in paleontology, and this was the time when the foundations of the science were laid. The period continued for six decades, or until the time of the Origin of Species. Marsh says that now "method replaced disorder, and systematic study superseded casual observation." Fossils were accurately determined, comparisons were made with living forms, and the species named according to the binomial system. However, every species, recent and extinct, was regarded as a separate creation, and because of the usually sharp separation of the superposed fossil faunas and floras, these were held to have been destroyed through a series of periodic catastrophes of which the Noachian deluge was the last.

Lamarck between 1802 and 1806 described the Tertiary shells of the Paris basin. Comparing them with the liv-

ing forms, he saw that most of the fossils were of extinct species, and in this way he came to be the founder of modern invertebrate paleontology. He also maintained after 1801 that life has been continuous since its origin and that nature has been uniform in the course of its development. Marsh adds:

"His researches on the invertebrate fossils of the Paris Basin, although less striking, were not less important than those of Cuvier on the vertebrates; while the conclusions he derived from them form the basis of modern biology."

"Lamarck was the prophetic genius, half a century in advance of his time."

Cuvier established comparative anatomy and vertebrate paleontology, and was one of the first to point out that fossil animals are nearly all extinct forms. He came to the latter conclusion in 1796 through a study of fossil elephants found in Europe. "Cuvier enriched the animal kingdom by the introduction of fossil forms among the living, bringing all together into one comprehensive system." This opened to him entirely new views respecting the theory of the earth, and he devoted more than twenty-five years to developing the theories of special creation and catastrophism, described in his Discourse on the Revolutions of the Surface of the Globe. "With all his knowledge of the earth, he could not free himself from tradition, and believed in the universality and power of the Mosaic deluge. Again, he refused to admit the evidence brought forward by his distinguished colleagues against the permanence of species, and used all his great influence to crush out the doctrine of evolution, then first proposed" (Marsh).

In England it was William Smith (1769-1839) who independently discovered the chronogenetic significance of fossils, and in their stratigraphic superposition indicated the way for the study of historical geology. He first published on this matter in 1799, but his completed statements came in works entitled "Strata identified by Organized Fossils," 1816-1820, and "Stratigraphical System of Organized Fossils," 1817.

Invertebrate paleontology in America during the Catastrophic period had its beginning in Lesueur, who

in 1818 described the Ordovician gastropod *Maclurites magna*. All of the paleontologists of this time were satisfied to describe species and genera and to ascertain in a broad way the stratigraphic significance of the fossil faunas and floras. James Hall in 1854 (**17**, 312) knew of 1588 species, described and undescribed, in the New York system, while in England Morris listed in that year 8300 Paleozoic forms. In 1856 Dana recites the known fossil species as follows (**22**, 333): The whole number of known American species of animals of the Permian to Recent is about 2000; while in Britain and Europe, there were over 20,000 species. In the Permian we have none, while Europe has over 200 species. In the Triassic we have none, Europe 1000 species; Jurassic 60, Europe over 4000; Cretaceous 350 to 400, Europe about 6000; Tertiary hardly 1500, Europe about 8000. Since that time nearly all of the larger American Paleozoic faunas have been developed, but there are thousands of species yet to be described. Who the more prominent American paleontologists of this period were has been told in the section on the development of the geological column.

The grander paleontologic results of the Catastrophic period have been so well stated by Marsh that it is worth our while to repeat them here:

"It had now been proved beyond question that portions at least of the earth's surface had been covered many times by the sea, with alternations of fresh water and of land; that the strata thus deposited were formed in succession, the lowest of the series being the oldest; that a distinct succession of animals and plants had inhabited the earth during the different geological periods; and that the order of succession found in one part of the earth was essentially the same in all. More than 30,000 new species of extinct animals and plants had now been described. It had been found, too, that from the oldest formations to the most recent, there had been an advance in the grade of life, both animal and vegetable, the oldest forms being among the simplest, and the higher forms successively making their appearance.

It had now become clearly evident, moreover, that the fossils from the older formations were all extinct species, and that only in the most recent deposits were there remains of forms still living . . . Another important conclusion reached, mainly through the labors of Lyell, was, that the earth had not been subjected in the past to sudden and violent revolutions; but the

great changes wrought had been gradual, differing in no essential respect from those still in progress. Strangely enough, the corollary to this proposition, that life, too, had been continuous on the earth, formed at that date no part of the common stock of knowledge. In the physical world, the great law of 'correlation of forces' had been announced, and widely accepted; but in the organic world, the dogma of the miraculous creation of each separate species still held sway."

Evolutionary Period.—This period begins with 1860 and the publication of Darwin's Origin of Species (late in 1859). It is the period of modern paleontology, and is dominated by the belief that universal laws pervade not only inorganic matter, but all life as well. Louis Agassiz had been in America fourteen years when Darwin's book appeared, and his wonderful influence in bringing the zoology of our country to a high stand and the further influence he exerted through his students was bound to react beneficially on invertebrate paleontology. Shortly after the beginning of this period, or in 1867, Alpheus Hyatt, one of Agassiz's students, began to apply the study of embryology to fossil cephalopods, showing clearly that these shells retain a great deal of their growth stages or ontogeny. This method of study was then followed by R. T. Jackson, C. E. Beecher, and J. P. Smith, and has been productive of natural classifications of the Cephalopoda, Brachiopoda, Trilobita, and Echinoidea.

The dominant invertebrate paleontologist of this period was of course James Hall, who described about 5000 species of American Paleozoic fossils. He also built up the New York State Museum, while around his private collections of fossils have been developed the American Museum of Natural History in New York City and the Walker Museum at the University of Chicago. In his most important laboratory of paleontology at Albany, there have been trained either wholly or in part the following paleontologists: F. B. Meek, C. A. White, R. P. Whitfield, C. D. Walcott, C. E. Beecher, John M. Clarke, and Charles Schuchert.

In Canada, through the work of the Geological Survey of the Dominion, came the paleontologists Elkanah Billings and, later on, J. F. Whiteaves. The "father of

Canadian paleontology," Sir William Dawson, who developed independently, was active in all branches of the science and did much to unravel the geology of eastern Canada. No organism has been more discussed and more often rejected and accepted as a fossil than his "dawn animal of Canada," *Eozoon canadense,* first described in 1865. His son, George M. Dawson, was one of the directors of the Geological Survey of Canada. Finally the extensive paleontology of the Cambrian of Canada was worked out by another self-made paleontologist, G. F. Matthew.

Palæobotany.—American paleobotany was developed during this, the fourth period, through the state and national surveys, first in Leo Lesquereux, a Swiss student induced by Agassiz to come to America, and in J. S. Newberry. The second generation of paleobotanists is represented by Lester F. Ward and W. N. Fontaine, and the third generation, the present workers, includes F. H. Knowlton, David White, Arthur Hollick, and E. W. Berry. A new line of paleobotanical work, the histology of woody but pseudomorphous remains, has been developed by G. R. Wieland.

The grander results of the study of paleontology during the evolutionary period may be summed up with the conclusions of Marsh:

> "One of the main characteristics of this epoch is the belief that all life, living and extinct, has been evolved from simple forms. Another prominent feature is the accepted fact of the great antiquity of the human race. These are quite sufficient to distinguish this period sharply from those that preceded it.
>
> Charles Darwin's work at once aroused attention, and brought about in scientific thought a revolution which "has influenced paleontology as extensively as any other department of science . . . In the [previous period] species were represented independently by parallel lines; in the present period, they are indicated by dependent, branching lines. The former was the analytic, the latter is the synthetic period."

Synthetic Period.—What is to be the next trend in paleontology? Clearly it is to be the Synthetic period, one that Marsh in 1879 indicated in these words: "But if we are permitted to continue in imagination the rapidly converging lines of research pursued to-day, they

seem to meet at the point where organic and inorganic nature become one. That this point will yet be reached, I cannot doubt."

This Synthetic period, foreshadowed also in Herbert Spencer's Synthetic Philosophy, has not yet arrived, but before long another great leader will appear. We have the prophecy of his coming in such books as The Fitness of the Environment, by Lawrence J. Henderson, 1913; The Origin and Nature of Life, by Benjamin Moore, 1913; The Organism as a Whole, by Jacques Loeb, 1916; and The Origin and Evolution of Life, by Henry F. Osborn, 1917.

In all nature, inorganic and organic, there is continuity and consistency, beauty and design. We are beginning to see that there are eternal laws, ever interacting and resulting in progressive and regressive evolutions. The realization of these scientific revelations kindles in us a desire for more knowledge, and the grandest revelations are yet before us in the synthesis of the sciences.

Notes.

[1] For more detail in regard to these tillites and the older ones see Climates of Geologic Time, by Charles Schuchert, being Chapter XXI in Huntington's Climatic Factor as Illustrated in Arid America, Publication No. 192 of the Carnegie Institution of Washington, 1914. Also Arthur P. Coleman's presidential address before the Geological Society of America in 1915, Dry Land in Geology, published in the Society's Bulletin, 27, 175, 1916.

III

A CENTURY OF GEOLOGY.—STEPS OF PROGRESS IN THE INTERPRETATION OF LAND FORMS

By HERBERT E. GREGORY

THE essence of physiography is the belief that land forms represent merely a stage in the orderly development of the earth's surface features; that the various dynamic agents perform their characteristic work throughout all geologic time. The formulation of principle and processes of earth sculpture was, therefore, impossible on the hypothesis of a ready-made earth whose features were substantially unchangeable, except when modified by catastrophic processes. In 1821, J. W. Wilson wrote in the Journal: "Is it not the best theory of the earth, that the Creator, in the beginning, at least at the general deluge, formed it with all its present grand characteristic features?"[1] If so, a search for causes is futile, and the study of the work performed by streams and glaciers and wind is unprofitable. The belief in the Deluge as the one great geological event in the history of the earth has brought it about that the speculations of Aristotle, Herodotus, Strabo, and Ovid, and the illustrious Arab, Avicenna (980-1037), unchecked by appeal to facts but also unopposed by priesthood or popular prejudice, are nearer to the truth than the intolerant controversial writings of the intellectual leaders whose touchstone was orthodoxy. A few thinkers of the sixteenth century revolted against the interminable repetition of error, and Peter Severinus (1571) advised his students: "Burn up your books . . . buy yourselves stout shoes, get away to the mountains, search the valleys, the deserts, the shores of the seas. . . . In this way and no

other will you arrive at a knowledge of things." But the thorough-going "diluvialist" who believed that a million species of animals could occupy a 450-foot Ark, but not that pebbles weathered from rock or that rivers erode, had no use for his powers of observation.

Sporadic germs of a science of land forms scattered through the literature of the seventeenth and eighteenth centuries found an unfavorable environment and produced inconspicuous growths. Even their sponsors did little to cultivate them. Steno (1631-1687) mildly suggested that surface sculpturing, particularly on a small scale, is largely the work of running water, and Guettard (1715-1786), a truly great mind, grasped the fundamental principles of denudation and successfully entombed his views as well as his reputation in scores of books and volumes of cumbrous diffuse writing.

At the beginning of the nineteenth century a sufficient body of principles had been established to justify the recognition of an earth science, geology, and the 195 volumes of the Journal thus far published carry a large part of the material which has won approval for the new science and given prominence to American thought. From the pages in the Journal, the progress of geology may be illustrated by tracing the fluctuation in the development of fact and theory as relates to valleys and glacial features, the subjects to which this chapter is devoted.

The Interpretation of Valleys.

The Pioneers.

Desmarest (1725-1815) might be styled the father of physiography. By concrete examples and sound induction he established (1774) the doctrine that the valleys of central France are formed by the streams which occupy them. He also made the first attempt to trace the history of a landscape through its successive stages on the basis of known causes. His methods and reasoning are practically identical with those of Dutton working in the ancient lavas of New Mexico; and Whitney's description of the Table Mountains of California might well have appeared in Desmarest's memoirs.[2] The teachings of Desmarest were strengthened and expanded by DeSaus-

sure (1740-1799), the sponsor for the term, "Geology," (1779) who saw in the intimate relation of Alpine streams and valleys the evidence of erosion by running water (1786).

The work of these acknowledged leaders of geological thought attracted singularly little attention on the Continent, and Lamarck's volume on denudation (Hydrogéologie), which appeared in 1802, although an important contribution, sank out of sight. But the seed of the French school found fertile ground in Edinburgh, the center of the geological world during the first quarter of the nineteenth century. Hutton's "Theory of the Earth, with Proofs and Illustrations," in which the guidance of DeSaussure and Desmarest is gratefully acknowledged, appeared in 1795. The original publication aroused only local interest, but when placed in attractive form by Playfair's "Illustrations of the Huttonian Theory" (1802), the problem of the origin and development of land forms assumed a commanding position in geological thought. Hutton was peculiarly fortunate in his environment. He had the support and assistance of a group of able scientific colleagues as well as the bitter opposition of Jameson and of the defenders of orthodoxy. His views were discussed in scientific publications and found their way to literary and theological journals. Hutton's conception of the processes of land sculpture—slow upheaving and slow degradation of mountains, differential weathering, and the carving of valleys by streams—has a very modern aspect. Playfair's book would scarcely be out of place in a twentieth century class room. The following paragraphs are quoted from it:[3]

" . . . A river, of which the course is both serpentine and deeply excavated in the rock, is among the phenomena, by which the slow waste of the land, and also the cause of that waste, are most directly pointed out.

The structure of the vallies among mountains, shews clearly to what cause their existence is to be ascribed. Here we have first a large valley, communicating directly with the plain, and winding between high ridges of mountains, while the river in the bottom of it descends over a surface, remarkable, in such a scene, for its uniform declivity. Into this, open a multitude of transverse or secondary vallies, intersecting the ridges on either

side of the former, each bringing a contribution to the main stream, proportioned to its magnitude; and, except where a cataract now and then intervenes, all having that nice adjustment in their levels, which is the more wonderful, the greater the irregularity of the surface. These secondary vallies have others of a smaller size opening into them; and, among mountains of the first order, where all is laid out on the greatest scale, these ramifications are continued to a fourth, and even a fifth, each diminishing in size as it increases in elevation, and as its supply of water is less. Through them all, this law is in general observed, that where a higher valley joins a lower one, of the two angles which it makes with the latter, that which is obtuse is always on the descending side; . . . what else but the water itself, working its way through obstacles of unequal resistance, could have opened or kept up a communication between the inequalities of an irregular and alpine surface . . .

. . . The probability of such a constitution [arrangement of valleys] having arisen from another cause, is, to the probability of its having arisen from the running of water, in such a proportion as unity bears to a number infinitely great.

. . .. With Dr. Hutton, we shall be disposed to consider those great chains of mountains, which traverse the surface of the globe, as cut out of masses vastly greater, and more lofty than any thing that now remains.

From this gradual change of lakes into rivers, it follows, that a lake is but a temporary and accidental condition of a river, which is every day approaching to its termination; and the truth of this is attested, not only by the lakes that have existed, but also by those that continue to exist."

Steps Backward.

Even Hutton's clear reasoning, firmly buttressed by concrete examples, was insufficient to overcome the belief in ready-made or violently formed valleys and original corrugations and irregularities of mountain surface. The pages of the Journal show that the principles laid down by Playfair were too far in advance of the times to secure general acceptance. In the first volume of the Journal, the gorge of the French Broad River is assigned by Kain to "some dreadful commotion in nature which probably shook these mountains to their bases,"[4] and the gorge of the lower Connecticut is considered by Hitchcock (1824)[5] as a breach which drained a series of lakes "not many centuries before the settlement of this

country." The prevailing American and English view for the first quarter of the nineteenth century is expressed in the reviews in this Journal, where the well-known conclusions of Conybeare and Phillips that streams are incompetent to excavate valleys are quoted with approval and admiration is expressed for Buckland's famous "Reliquiæ Diluvianæ," a 300-page quarto volume devoted to proof of a deluge. The professor at Yale, Silliman, and the professor at Oxford, Buckland, saw that an acceptance of Hutton's views involved a repudiation of the Biblical flood, and much space is devoted to combating these "erroneous" and "unscientific" views. For example, Buckland says:[6]

". . . The general belief is, that existing streams, avalanches and lakes, bursting their barriers, are sufficient to account for all their phenomena, and not a few geologists, especially those of the Huttonian school, at whose head is Professor Playfair, have till recently been of this opinion. . . . But it is now very clear to almost every man, who impartially examines the facts in regard to existing vallies, that the causes now in action, mentioned above, are altogether inadequate to their production; nay, that such a supposition would involve a physical impossibility. We do not believe that one-thousandth part of our present vallies were excavated by the power of existing streams. . . . In very many cases of large rivers, it is found, that so far from having formed their own beds, they are actually in a gradual manner filling them up.

Again; how happens it that the source of a river is frequently below the head of a valley, if the river excavated that valley?

The most powerful argument, however, in our opinion, against the supposition we are combating, is the phenomena of transverse and longitudinal valleys; both of which could not possibly have been formed by existing streams."

Phillips writes in 1829:[7] "The excavation of valleys can be ascribed to no other cause than a great flood of water which overtopped the hills, whose summits those vallies descend."

Faith in Noah's flood as the dominant agent of erosion rapidly lost ground through the teaching of Lyell after 1830, but the theory of systematic development of landscapes by rivers gained little. In fact, Scrope in 1830,[8] in showing that the entrenched meanders of the Moselle

prove gradual progressive stream work, was in advance of his English contemporary. Judged by contributions to the Journal, Lyell's teaching served to standardize American opinion of earth sculpture somewhat as follows: The ocean is the great valley maker, but rivers also make them; the position of valleys is determined by original or renewed surface inequalities or by faulting; exceptional occurrences—earthquakes, bursting of lakes, upheavals and depressions—have played an important part. Hayes (1839)[9] thought that the surface of New York was essentially an upraised sea-bottom modified by erosion of waves and ocean currents. Sedgwick (1838)[10] considered high-lying lake basins proof of valleys which were shaped under the sea. Many of the valleys in the Chilian Cordillera were thought by Darwin (1844) to have been the work of waves and tides, and water gaps are ascribed to currents "bursting through the range at those points where the strata have been least inclined and the height consequently is less." Speaking of the magnificent stream-cut canyons of the Blue Mountains of New South Wales, gorges which lead to narrow exits through monoclines, Darwin says: "To attribute these hollows to alluvial action would be preposterous."[11]

The influence of structure in the formation of valleys is emphasized by many contributors to the Journal. Hildreth in 1836, in a valuable paper,[12] which is perhaps the first detailed topographic description of drainage in folded strata, expresses the opinion that the West Virginia ridges and valleys antedated the streams and that water gaps though cut by rivers involve pre-existing lakes. Geddes (1826)[13] denied that Niagara River cut its channel and speaks of valleys which "were valleys e'er moving spirit bade the waters flow." Conrad (1839)[14] discussed the structural control of the Mohawk, the Ohio, and the Mississippi, and Lieutenant Warren (1859)[15] concluded that the Niobrara must have originated in a fissure. According to Lesley (1862)[16] the course of the New River across the Great Valley and into the Appalachians "striking the escarpment in the face" is determined by the junction of anticlinal structures on the north with faulted monoclines toward the south; a conclusion in harmony

with the views of Edward Hitchcock (1841)[17] that major valleys and mountain passes are structural in origin and that even subordinate folds and faults may determine minor features. "Is not this a beautiful example of prospective benevolence on the part of the Deity, thus, by means of a violent fracture of primary mountains, to provide for easy intercommunication through alpine regions, countless ages afterwards!" The extent of the wandering from the guidance of DeSaussure and Playfair after the lapse of 50 years is shown by students of Switzerland. Alpine valleys to Murchison (1851) were bays of an ancient sea; Schlaginweit (1852) found regional and local complicated crustal movements a satisfactory cause, and Forbes (1863) saw only glaciers.

Valleys Formed by Rivers.

One strong voice before 1860 appears to have called Americans back to truths expounded by Desmarest and Hutton. Dana in 1850[18] amply demonstrated that valleys on the Pacific Islands owe neither their origin, position or form to the sea or to structural factors. They are the work of existing streams which have eaten their way headwards. Even the valleys of Australia cited by Darwin as type examples of ocean work are shown to be products of normal stream work. Dana went further and gave a permanent place to the Huttonian idea that many bays, inlets, and fiords are but the drowned mouths of stream-made valleys. In the same volume in which these conclusions appeared, Hubbard (1850)[19] announced that in New Hampshire the "deepest valleys are but valleys of erosion." The theory that valleys are excavated by streams which occupy them was all but universally accepted after F. V. Hayden's description[20] of Rocky Mountain gorges (1862) and Newberry's interpretation of the canyons of Arizona (1862); but the scientific world was poorly prepared for Newberry's statement:[21]

"Like the great canons of the Colorado, the broad valleys bounded by high and perpendicular walls *belong to a vast system of erosion, and are wholly due to the action of water. . . .* The first and most plausible explanation of the striking surface features of this region will be to refer them to that embodiment of

resistless power—the sword that cuts so many geological knots—volcanic force. The Great Cañon of the Colorado would be considered a vast fissure or rent in the earth's crust, and the abrupt termination of the steps of the table lands as marking lines of displacement. This theory though so plausible, and so entirely adequate to explain all the striking phenomena, lacks a single requisite to acceptance, and that is *truth.*"

With such stupendous examples in mind, the dictum of Hutton seemed reasonable: "there is no spot on which rivers may not formerly have run."

Denudation by Rivers.

The general recognition of the competency of streams to form valleys was a necessary prelude to the broader view expressed by Jukes (1862)[22]

"The surfaces of our present lands are as much carved and sculptured surfaces as the medallion carved from the slab, or the statue sculptured from the block. They have been gradually reached by the removal of the rock that once covered them, and are themselves but of transient duration, always slowly wasting from decay."

Contributions to the Journal between 1850 and 1870 reveal a tendency to accept greater degrees of erosion by rivers, but the necessary end-product of subaërial erosion—a plain—is first clearly defined by Powell in 1875.[23] In formulating his ideas Powell introduced the term "base-level," which may be called the germ word out of which has grown the "cycle of erosion," the master key of modern physiographers. The original definition of base-level follows:

"We may consider the level of the sea to be a grand base-level, below which the dry lands cannot be eroded; but we may also have, for local and temporary purposes, other base-levels of erosion, which are the levels of the beds of the principal streams which carry away the products of erosion. (I take some liberty in using the term 'level' in this connection, as the action of a running stream in wearing its channel ceases, for all practical purposes, before its bed has quite reached the level of the lower end of the stream. What I have called the base-level would, in fact, be an imaginary surface, inclining slightly in all its parts toward the lower end of the principal stream draining the area

through which the level is supposed to extend, or having the inclination of its parts varied in direction as determined by tributary streams.)"

Analysis of Powell's view has given definiteness to the distinction between "base-level," an imaginary plane, and "a nearly featureless plain," the actual land surface produced in the last stage of subaërial erosion.

Following their discovery in the Colorado Plateau Province, denudation surfaces were recognized on the Atlantic slope and discussed by McGee (1888),[24] in a paper notable for the demonstration of the use of physiographic methods and criteria in the solution of stratigraphic problems. Davis (1889)[25] described the upland of southern New England developed during Cretaceous time, introducing the term "peneplain," "a nearly featureless plain." The short-lived opposition to the theory of peneplanation indicates that in America at least the idea needed only formulation to insure acceptance.

It is interesting to note that surfaces now classed as peneplains were fully described by Percival (1842),[26] who assigned them to structure, and by Kerr (1880),[27] who considered glaciers the agent. In Europe "plains of denudation" have been clearly recognized by Ramsay (1846), Jukes (1862), A. Geikie (1865), Foster and Topley (1865), Maw (1866), Wynne (1867), Whitaker (1867), Macintosh (1869), Green (1882), Richthofen (1882), but all of them were looked upon as products of marine work, and writers of more recent date in England seem reluctant to give a subordinate place to the erosive power of waves. Americans, on the other hand, have been thinking in terms of rivers, and the great contribution of the American school is not that peneplains exist, but that they are the result of normal subaërial erosion. More precise field methods during the past decade have revealed the fact that no one agent is responsible for the land forms classed as peneplains; that not only rivers and ocean, but ice, wind, structure, and topographic position must be taken into account.

The recognition of rivers as valley-makers and of the final result of stream work necessarily preceded an analysis of the process of subaërial erosion. The first

and last terms were known, the intermediate terms and the sequence remained to be established. A significant contribution to this problem was made by Jukes (1862).[22]

". . . I believe that the lateral valleys are those which were first formed by the drainage running directly from the crests of the chains, the longitudinal ones being subsequently elaborated along the strike of the softer or more erodable beds exposed on the flanks of those chains."

Powell's discussion of antecedent and consequent drainage (1875) and Gilbert's chapter on land sculpture in the Henry Mountain report (1880) are classics, and McGee's contribution[28] contains significant suggestions. but the master papers are by Davis,[29] who introduces an analysis of land forms based on structure and age by the statement:

"Being fully persuaded of the gradual and systematic evolution of topographical forms it is now desired . . . to seek the causes of the location of streams in their present courses; to go back if possible to the early date when central Pennsylvania was first raised from the sea, and trace the development of the several river systems then implanted upon it from their ancient beginning to the present time."

That such a task could have been undertaken a quarter of a century ago and to-day considered a part of everyday field work shows how completely the lost ground of a half-century has been regained and how rapid the advance in the knowledge of land sculpture since the canyons of the Colorado Plateau were interpreted.

Features Resulting from Glaciation.

The Problem Stated.

Early in the nineteenth century when speculation regarding the interior of the earth gave place in part to observations of the surface of the earth, geologists were confronted with perhaps the most difficult problem in the history of the science. As stated by the editor of the Journal in 1821:[30]

"The almost universal existence of rolled pebbles, and boulders of rock, not only on the margin of the oceans, seas, lakes, and rivers; but their existence, often in enormous quantities, in

situations quite removed from large waters; inland,—in high banks, imbedded in strata, or scattered, occasionally, in profusion, on the face of almost every region, and sometimes on the tops and declivities of mountains, as well as in the vallies between them; their entire difference, in many cases, from the rocks in the country where they lie—rounded masses and pebbles of primitive rocks being deposited in secondary and alluvial regions, and vice versa; these and a multitude of similar facts have ever struck us as being among the most interesting of geological occurrences, and as being very inadequately accounted for by existing theories."

The phenomena demanding explanation—jumbled masses of "diluvium," polished and striated rock, bowlders distributed with apparent disregard of topography—were indeed startling. Even Lyell, the great exponent of uniformitarianism, appears to have lost faith in his theories when confronted with facts for which known causes seemed inadequate. The interest aroused is attested by 31 titles in the Journal during its first two decades, articles which include speculations unsupported by logic or fact, field observation unaccompanied by explanation, field observation with fantastic explanation, *ex-cathedra* pronouncements by prominent men, sound reasoning from insufficient data, and unclouded recognition of cause and effect by both obscure and prominent men. With little knowledge of glaciers, areal geology, or of structure and composition of drift, all known forces were called in: normal weathering, catastrophic floods, ocean currents, waves, icebergs, glaciers, wind, and even depositions from a primordial atmosphere (Chabier, 1823). Human agencies were not discarded. Speaking of a granite bowlder at North Salem, New York, described by Cornelius (1820)[31] as resting on limestone, Finch (1824)[32] says: "it is a magnificent cromlech and the most ancient and venerable monument which America possesses." In the absence of a known cause, catastrophic agencies seem reasonable.

The Deluge.

In the seventh volume of the Journal (1824)[33] we read:

"After the production of these regular strata of sand, clay, limestone, &c. came a terrible irruption of water from the north,

or north-west, which in many places covered the preceding formations with diluvial gravel, and carried along with it those immense masses of granite, and the older rocks, which attest to the present day the destruction and ruin of a former world."

Another author remarks:

"We find a mantle as it were of sand and gravel indifferently covering all the solid strata, and evidently derived from some convulsion which has lacerated and partly broken up those strata. . . ."

The catastrophe favored by most geologists was floods of water violently released—"we believe," says the editor, "that all geologists agree in imputing . . . the diluvium to the agency of a deluge at one period or another."[34] Such conclusions rested in no small way upon Hayden's well-known treatise on surficial deposits (1821),[35] a volume which deserves a prominent place in American geological literature. Hayden clearly distinguished the topographic and structural features of the drift but found an adequate cause in general wide-spread currents which "flowed impetuously across the whole continent... from north east to south west." In reviewing Hayden's book Silliman remarks:

"The general cause of these currents Mr. Hayden concludes to be the deluge of Noah. While no one will object to the propriety of ascribing very many, probably most of our alluvial features, to that catastrophe, we conceive that neither Mr. Hayden, nor any other man, is bound to prove the immediate physical cause of that vindictive infliction.

We would beg leave to suggest the following as a cause which *may* have aided in deluging the earth, and which, were there occasion, *might* do it again.

The existence of enormous caverns in the bowels of the earth, (so often imagined by authors,) appears to be no very extravagant assumption. It is true it cannot be proved, but in a sphere of eight thousand miles in diameter, it would appear in no way extraordinary, that many cavities might exist, which collectively, or even singly, might well contain much more than all our oceans, seas, and other superficial waters, none of which are probably more than a few miles in depth. If these cavities communicate in any manner with the oceans, and are (as if they exist at all, they probably are,) filled with water, there exist, we

conceive, agents very competent to expel the water of these cavities, and thus to deluge, at any time, the dry land."

The teachings of Hayden were favorably received by Hitchcock, Struder, and Hubbard, and many Europeans. They found a champion in Jackson, who states (1839):[36]

"From the observations made upon Mount Ktaadn, it is proved, that the current did rush over the summit of that lofty mountain, and consequently the diluvial waters rose to the height of more than 5,000 feet. Hence we are enabled to prove, that the ancient ocean, which rushed over the surface of the State, was at least a mile in depth, and its transporting power must have been greatly increased by its enormous pressure."

Gibson, a student of western geology, reaches the same conclusion (1836):[37]

"That a wide-spread current, although not, as imagined, fed from an inland sea, once swept over the entire region between the Alleghany and the Rocky Mountains is established by plenary proof."

Professor Sedgwick (1831) thought the sudden upheaval of mountains sufficient to have caused floods again and again. The strength of the belief in the Biblical flood, during the first quarter of the 19th century, may be represented by the following remarks of Phillips (1832):[38]

"Of many important facts which come under the consideration of geologists, the 'Deluge' is, perhaps, the most remarkable; and it is established by such clear and positive arguments, that if any one point of natural history may be considered as proved, the deluge must be admitted to have happened, because it has left full evidence in plain and characteristic effects *upon the surface of the earth.*"

However, the theory of deluges, whether of ocean or land streams, did not hold the field unopposed. In 1823, Granger,[39] an observer whose contributions to science total only six pages, speaks of the striæ on the shore of Lake Erie as

"having been formed by the powerful and continued attrition of some hard body. . . . To me, it does not seem possible that water under any circumstances, could have effected it. The flutings in

width, depth, and direction, are as regular as if they had been cut out by a grooving plane. This, running water could not effect, nor could its operation have produced that glassy smoothness, which, in many parts, it still retains."

Hayes and also Conrad expressed similar views in the Journal 16 years later.

The idea that ice was in some way concerned with the transportation of drift has had a curious history. The first unequivocal statement, based on reading and keen observation, was made in the Journal by Dobson in 1826:[40]

"I have had occasion to dig up a great number of bowlders, of red sandstone, and of the conglomerate kind, in erecting a cotton manufactory; and it was not uncommon to find them worn smooth on the under side, as if done by their having been dragged over rocks and gravelly earth, in one steady position. On examination, they exhibit scratches and furrows on the abraded part; and if among the minerals composing the rock, there happened to be pebbles of feldspar, or quartz, (which was not uncommon,) they usually appeared not to be worn so much as the rest of the stone, preserving their more tender parts in a ridge, extending some inches. When several of these pebbles happen to be in one block, the preserved ridges were on the same side of the pebbles, so that it is easy to determine which part of the stone moved forward, in the act of wearing.

These bowlders are found, not only on the surface, but I have discovered them a number of feet deep, in the earth, in the hard compound of clay, sand, and gravel. . . .

I think we cannot account for these appearances, unless we call in the aid of ice along with water, and that they have been worn by being suspended and carried in ice, over rocks and earth, under water."

In Dobson's day the hypothesis of "gigantic floods," "debacles," "resistless world-wide currents," was so firmly entrenched that the voice of the observant layman found no hearers, and a letter from Dobson to Hitchcock written in 1837 and containing additional evidence and argument remained unpublished until Murchison, in 1842,[41] paid his respects to the remarkable work of a remarkable man.*

* Peter Dobson (1784-1878) came to this country from Preston, England, in 1809 and established a cotton factory at Vernon, Conn.

"I take leave of the glacial theory in congratulating American science in having possessed the original author of the best glacial theory, though his name had escaped notice; and in recommending to you the terse argument of Peter Dobson, a previous acquaintance with which might have saved volumes of disputation on both sides of the Atlantic."

Glaciers vs. Icebergs.

The glacial theory makes its way into geological literature with the development of Agassiz (1837) of the views of Venetz (1833) and Charpentier (1834), that the glaciers of the Alps once had greater extent. The bold assumption was made that the surface of Europe as far south as the shores of the Mediterranean and Caspian seas was covered by ice during a period immediately preceding the present. The kernel of the present glacial theory is readily recognizable in these early works, but it is wrapped in a strange husk: it was assumed that the Alps were raised by a great convulsion under the ice and that the erratics slid to their places over the newly made declivities. The publication of the famous "Etudes sur les Glaciers" (1840), remarkable alike for its clarity, its sound inductions, and wealth of illustrations, brought the ideas of Agassiz more into prominence and inaugurated a 30-years' war with the proponents of currents and icebergs. The outstanding objections to the theory were the requirement of a frigid climate and the demand for glaciers of continental dimensions; very strong objections, indeed, for the time when fossil evidence was not available, the great polar ice sheets were unexplored, and the distinction between till and water-laid drift had not been established.

The glacial theory was cordially adopted by Buckland (1841)[42] and in part by Lyell in England but viewed with suspicion by Sedgwick, Whewell, and Mantell. In America the response to the new idea was immediate. Hitchcock (1841)[17] concludes an able discussion with the statement: "So remarkably does it solve most of the phenomena of diluvial action, that I am constrained to believe its fundamental principles to be founded in truth."

The theory formed the chief topic of discussion at the

third and fourth meetings of the Association of American Geologists and Naturalists (1842, 1843) under the lead of a committee on drift consisting of Emmons, W. B. Rogers, Vanuxem, Nicollet, Jackson, and J. L. Hayes. The result of these discussions was a curious reaction. Hitchcock complained that he "had been supposed to be an advocate for the unmodified glacial theory, but he had never been a believer in it," and Jackson spoke for a number of men when he stated:[43]

"This country exhibits no proofs of the glacial theory as taught by Agassiz but on the contrary the general bearing of the facts is against that theory. . . . Many eminent men incautiously embraced the new theory, which within two or three years from its promulgation, had been found utterly inadequate, and is now abandoned by many of its former supporters."

Out of this symposium came also the strange contribution of H. D. Rogers (1844),[44] who cast aside the teachings of deduction and observation and returned to the views of the Medievalists.

"If we will conceive, then, a wide expanse of waters, less perhaps than one thousand feet in depth, dislodged from some high northern or circumpolar basin, by a general lifting of that region of perhaps a few hundred feet, and an equal subsidence of the country south, and imagine this whole mass converted by earthquake pulsations of the breadth which such undulations have, into a series of stupendous and rapid-moving waves of translation, helped on by the still more rapid flexures of the floor over which they move, and then advert to the shattering and loosening power of the tremendous jar of the earthquake, we shall have an agent adequate in every way to produce the results we see, to float the northern ice from its moorings, to rip off, assisted with its aid, the outcrops of the hardest strata, to grind up and strew wide their fragments, to scour down the whole rocky floor, and, gathering energy with resistance, to sweep up the slopes and over the highest mountains."

Because of the prominence of their author, Rogers's views exerted some influence and seemingly received support from England through the elaborate mathematic discussions of Whewell (1848), who considered the drift as "irresistible proof of paroxysmal action," and Hopkins (1852), who contended for "currents produced by repeated elevatory movements."

After his arrival in America (1846), Agassiz's influence was felt, and his paper on the erratic phenomena about Lake Superior (1850),[45] in which he called upon the advocates of water-borne ice to point out the barrier which caused the current to subside, produced a salutary effect; yet Desor (1852)[46] states that in the region described by Agassiz "the assumption [of a general ice cap] is no longer admissible," and that the bowlders on Long Island "were transported on ice rafts along the sea shore and stranded on the ridges and eminences which were then shoals along the coast." Twenty years of discussion were insufficient to establish the glacial theory either in Europe or America. The consensus of opinion among the more advanced thinkers in 1860 is expressed by Dana:[47]

"In view of the whole subject, it appears reasonable to conclude that the Glacier theory affords the best and fullest explanation of the phenomena over the general surface of the continents, and encounters the fewest difficulties. But icebergs have aided beyond doubt in producing the results along the borders of the continents, across ocean-channels like the German Ocean and the Baltic, and possibly over great lakes like those of North America. Long Island Sound is so narrow that a glacier may have stretched across it."

Papers in the Journal of 1860-70 show a prevailing belief in icebergs, but the evidence for land ice was accumulating as the deposits became better known, and in 1871 field workers speak in unmistakable tones:[48]

"It is still a mooted question in American geology whether the events of the Glacial era were due to *glaciers* or *icebergs*. . . . American geologists are still divided in opinion, and some of the most eminent have pronounced in favor of icebergs.

Since, then, icebergs cannot pick up masses tons in weight from the bottom of a sea, or give a general movement southward to the loose material of the surface; neither can produce the abrasion observed over the rocks under its various conditions; and inasmuch as all direct evidence of the submergence of the land required for an iceberg sea over New England fails, the conclusion appears inevitable that icebergs had nothing to do with the drift of the New Haven region, in the Connecticut valley; and, therefore, that the Glacial era in central New England was a *Glacier* era."

Matthew (1871)[49] reached the same conclusion for the Lower Provinces of Canada. In spite of the increasing clarity of the evidence, the battle for the glacial theory was not yet won. The remaining opponents though few in number were distinguished in attainments. Dawson clung to the outworn doctrine until his death in 1899.

An interesting feature of the history of glacial theories is the calculation by Maclaren (1842)[50] that the amount of water abstracted from the seas to form the hypothetical ice sheet would lower the ocean-level 350 feet—an early form of the glacial control hypothesis (see Daly[51]).

Extent of Glacial Drift.

By the middle of the nineteenth century, it was recognized that the "drift," whatever its origin, was not of world-wide extent. In America its characteristic features were found best developed north of latitude 40 degrees; in Europe, the Alps, the Scottish Highlands, and Scandinavia were recognized as type areas. The limits were unassigned, partly because the field had not been surveyed, but largely because criteria for the recognition of drift had not been established. The well-known hillocks and ridges of "diluvium" and "alluvium" and "drift" of New Jersey and Ohio, and the mounds of the Missouri Cotou elaborately described by Catlin (1840)[52] bore little resemblance to the walls of unsorted rock which stand as moraines bordering Alpine glaciers. The Orange sand of Mississippi was included in the drift by Hilgard (1866),[53] and the gravels at Philadelphia by Hall (1876).[54] Stevens (1873)[55] described trains of glacial erratics at Richmond, Virginia, and William B. Rogers (1876)[56] accounts for certain deposits in the Potomac, James, and Roanoke rivers by the presence of Pleistocene ice tongues or swollen glacial rivers, and remarks: "It is highly probable that glacial action had much to do with the original accumulation of the rocky debris on the flanks of the Blue Ridge, and in the Appalachian valleys beyond." Kerr (1881)[57] referred the ancient erosion surface of the Piedmont belt in North Carolina to glacial denudation, De la Beche compared

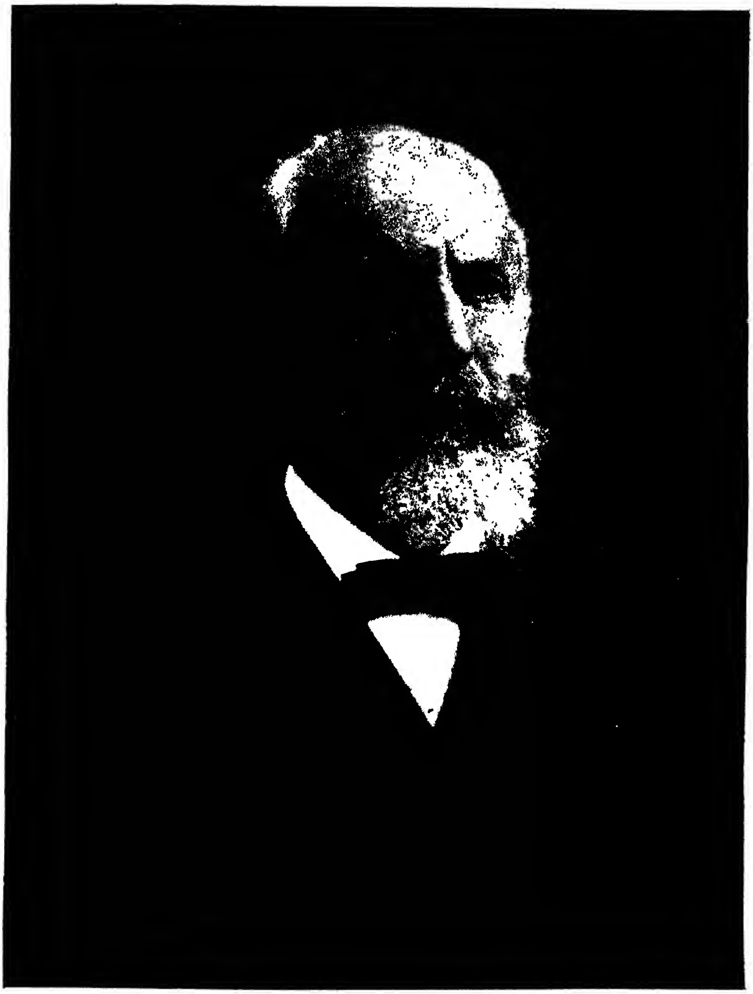
the drift of Jamaica with that of New England, and Agassiz interpreted soils of Brazil as glacial.

The first detailed description and unequivocal interpretation of either terminal or recessional moraines is from the pen of Gilbert (1871),[58] geologist of the Ohio Survey. In discussing the former outlet of Lake Erie through the Fort Wayne channel, Gilbert writes:

"The page of history recorded in these phenomena is by no means ambiguous. The ridges, or, more properly, the ridge which determines the courses of the St. Joseph and St. Marys rivers is a buried terminal moraine of the glacier that moved southwestward through the Maumee valley. The overlying Erie Clay covers it from sight, but it is shadowed forth on the surface of that deposit, as the ground is pictured through a deep and even canopy of snow. Its irregularly curved outline accords intimately with the configuration of the valley, and with the direction of the ice markings; its concavity is turned toward the source of motion; its greatest convexity is along the line of least resistance.

South of the St. Marys river are other and numerous moraines accompanied by glacial striæ. Their character and courses have not yet been studied; but their presence carries the mind back to an epoch of the cold period, when the margin of the ice-field was farther south, and the glacier of the Maumee valley was merged in the general mass. As the mantle of ice grew shorter—and, in fact, at every stage of its existence—its margin must have been variously notched and lobed in conformity with the contour of the country, the higher lands being first laid bare by the encroaching secular summer. Early in the history of this encroachment the glacier of the Maumee valley constituted one of these lobes, and has recorded its form in the two moraines that I have described."

Three years after the recognition of moraines in the Maumee valley, Chamberlin (1874)[59] showed that the seemingly disorganized mounds and basins and ridges known as the Kettle range of Wisconsin is the terminal moraine of the Green Bay glacier. At an earlier date (1864) Whittlesey interpreted the kettles of the Wisconsin moraine as evidence of ice blocks from a melting glacier and presented a map showing the "southern limit of boulders and coarse drift." In 1876 attention was called to the terminal moraine of New England by G.



G. K. Gilbert

Frederick Wright, who assigns the honor of discovery to Clarence King.

With the observations of Gilbert, Chamberlin, and King in mind, the terminal moraine was traced by various workers across the United States and into Canada and the extent of glacial cover revealed. Following 1875 the pages of the Journal contain many contributions dealing with the origin and structure of moraines, eskers, kames, and drumlins. Before 1890 twenty-eight papers on the glacial phenomena of the Erie and Ohio basin alone had appeared. By 1900 substantial agreement had been reached regarding the significant features of the drift, the outline history of the Great Lakes had been written, and the way had been paved for stratigraphic studies of the Pleistocene, which bulk large in the pages of the Journal for the last two decades.

Epochs of Glaciation.

For a decade following the general acceptance of the glacial origin of "diluvium," the deposits were embraced as "drift" and treated as the products of one long period of glacial activity, and throughout the controversy of iceberg and glacier the unity of the glacial period was unquestioned. Beds of peat and fossiliferous lacustrine deposits in Switzerland, England, and in America and the recognition of an "upper" and a "lower" diluvium by Scandinavian geologists suggested two epochs, and as the examples of such deposits increased in number and it became evident that the plant fossils represented forms demanding a genial climate and that the phenomena were seen in many countries, the belief grew that minor fluctuations or gradual recession of an ice sheet were inadequate to account for the phenomena observed.

It is natural that this problem should have found its solution in America, where the Pleistocene is admirably displayed, and where the State and Federal surveys were actively engaged in areal mapping. In 1883 Chamberlin[60] presented his views under the bold title, "Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch," and the existence of deposits of two or more ice sheets and the features of interglacial periods

were substantially established by the interesting debate in the Journal led by Chamberlin, Wright, Upham and Dana.[61] Contributions since 1895 have been concerned with the degree rather than the fact of complexity, and continued study has resulted in the general recognition of five glacial stages in North America and four in Europe.

The Loess as a Glacial Deposit.

A curious side-product of the study of glaciation in North America is the controversy over the origin of loess. The interest aroused is indicated by scores of papers in American periodicals and State reports of the last quarter of the 19th century—papers which bear the names of prominent geologists.

The "loess" in the valley of the Rhine had long been known, but the subject assumed prominence by the publication in 1866 of Pumpelly's Travels in China.[62] Widespread deposits 200 to 1,000 feet thick were described as very fine-grained yellowish earth of distinctive structure without stratification but penetrated by innumerable tubes and containing land or fresh-water shells. Pumpelly considered these deposits lacustrine, a view which found general acceptance though combated by Kingsmill (1871),[63] who argued for marine deposition. Baron Von Richthofen's classic on China, which appeared in 1877, amplifies the observations of Pumpelly and marshals the evidence to support the hypothesis that the loess is wind-laid both on dry land and within ancient salt lakes. The conclusions of Von Richthofen were adopted by Pumpelly whose knowledge of the Chinese deposits, supplemented by studies in Missouri, of which State he was director of the Geological Survey in 1872-73, placed him in position to form a correct judgment. He says:[64]

"Recognizing from personal observation the full identity of character of the loess of northern China, Europe and the Missouri Valley, I am obliged to reject my own explanation of the origin of the Chinese deposits, and to believe with Richthofen that the true loess, wherever it occurs, is a sub-aerial deposit, formed in a dry central region, and that it owes its structure to the formative influence of a steppe vegetation.

The one weak point of Richthofen's theory is in the evident

inadequacy of the current disintegration as a source of material. When we consider the immense area covered by loess to depths varying from 50 to 2,000 feet, and the fact that this is only the very finest portion of the product of rock-destruction, and again that the accumulation represents only a very short period of time, geologically speaking, surely we must seek a more fertile source of supply than is furnished by the current decomposition of rock surface.

It seems to me that there are two important sources: I. The silt brought by rivers, many of them fed by the products of glacial attrition flowing from the mountains into the central region. Where the streams sink away, or where the lakes which receive them have dried up, the finer products of the erosion of a large territory are left to be removed in dust storms.

II. The second . . . source is the residuary products of a secular disintegration.''

The evidence presented by Pumpelly for the eolian origin of loess—structure, texture, composition, fossil content and topographic position—is complete, and to him belongs the credit for the correct interpretation of the Mississippi valley deposits. Unfortunately his contribution came at a time when the geologists of the central States were intent on tracing the paths and explaining the work of Pleistocene glaciers, and the belief was strong that loess was some phase of glacial work. Its position at the border of the Iowan drift so obviously suggests a genetic relation that the fossil evidence of steppe climate suggested by Binney in 1848[65] was minimized. Students of Pleistocene geology in Minnesota, Iowa, Nebraska, Missouri, although less vigorous in expression, were substantially in agreement with Hilgard (1879).[66] "The sum total of anomalous conditions required to sustain the eolian hypothesis partakes strongly of the marvellous.'' The last edition of Dana's Manual, 1894, and of LeConte's Geology, 1896, the two most widely used text books of their time, oppose the eolian theory, and Chamberlin, in 1897,[67] states: "the aqueous hypothesis seems best supported so far as concerns the deposits of the Mississippi Valley and western Europe'' (p. 795). Shimek, in papers published since 1896 has shown that aquatic and glacial conditions can not account for the loess fossils, and the return to the views of Pumpelly that the loess was deposited on land

by the agency of wind in a region of steppe vegetation is now all but universal.

Glacial Sculpture.

Within the present generation sculpture by glaciers has received much attention and has involved a reconsideration of the ability of ice to erode which in turn involves a crystallization of views of the mechanics of moving ice. The evidence for glacier erosion has remained largely physiographic and rests on a study of land forms. In fact, the inadequacy of structural features or of river corrasion to account for flat-floored, steep-walled gorges, hanging valleys, and many lake basins, rather than a knowledge of the mechanics of ice has led to the present fairly general belief that glaciers are powerful agents of rock sculpture. The details of the process are not yet understood.

Erosion by glaciers enters the arena of active discussion in 1862-63. The possibility had been suggested by Esmark (1827) and by Dana (1849) in the description of fiords and by Hind (1855) with reference to the origin of the Great Lakes. It appears full-fledged in Ramsay's classic, which was published simultaneously in England and in America.[68] The argument runs as follows: There is a close association of ancient glaciers and lakes especially in mountains; glaciers are amply able to erode; evidences of faulting, special subsidence, river erosion, and marine erosion are absent from the lake basins of Switzerland and Great Britain. To quote Ramsay:

"It required a solid body grinding steadily and powerfully in direct and heavy contact with and across the rocks to scoop out deep hollows, the situations of which might either be determined by unequal hardness of the rocks, by extra weight of ice in special places, or by accidental circumstances, the clue to which is lost from our inability perfectly to reconstruct the original forms of the glaciers."

"I believe with the Italian geologists, that all that the glaciers as a whole effected was only slightly to deepen these valleys and materially to modify their general outlines, and, further (a theory I am alone responsible for), to deepen them in parts more considerably when, from various causes, the grinding power of

the ice was unusually powerful, especially where, as in the lowlands of Switzerland, the Miocene strata are comparatively soft."

Whittlesey (1864)[69] considered that the rock-bound lakes and narrow bays near Lake Superior were partly excavated by ice. LeConte (1875)[70] records some significant observations in a pioneer paper on glacier erosion which has not received adequate recognition. He says:

". . . I am convinced that a glacier, by its enormous pressure and resistless onward movement, is *constantly breaking off large blocks* from its bed and bounding walls. Its erosion is not only a grinding and scoring, but also a *crushing and breaking.* It makes by its erosion not only rock-meal, but also large *rock-chips.* . . . Its erosion is a constant process of alternate *rough hewing* and *planing.*

If Yosemite were unique, we might suppose that it was formed by violent cataclysms; but *Yosemite is not unique in form* and therefore probably not in *origin.* There are many Yosemites. It is more philosophical to account for them by the *regular* operation of known causes. I must believe that all these deep perpendicular slots have been sawn out by the action of glaciers; the *peculiar verticality of the walls having been determined by the perpendicular cleavage structure."* . . . A lake in Bloody Canyon "is a *pure rock basin scooped out by the glacier* at this place. . . . These ridges [separating Hope, Faith, and Charity valleys] are in fact the lips of consecutive lake basins scooped out by ice.

. . . Water tends to form deep V-shaped canons, while ice produces broad valleys with lakes and meadows. . . . I know not how general these distinctions may be, but certainly the Coast range of this State is characterized by rounded summits and ridges, and deep V-shaped canons, while the high Sierras are characterized on the contrary by sharp, spire-like, comb-like summits, and broad valleys; and this difference I am convinced is due in part at least to the action of water on the one hand, and of ice on the other."

King (1878)[71] assigned to glacial erosion a commanding position in mountain sculpture. In regard to the Uintas, he says:

"Glacial erosion has cut almost vertically down through the beds carving immense amphitheatres with basin bottoms containing numerous Alpine lakes. . . . Post-glacial erosion has done

an absolutely trivial work. There is not a particle of direct evidence, so far as I can see, to warrant the belief that these U-shaped canons were given their peculiar form by other means than the actual ploughing erosion of glaciers. . . .''

These contributions from the Cordilleras corroborating the conclusions of Ramsay (1862), Tyndall (1862), Jukes (1862), Hector (1863), Logan (1863), Close (1870), and James Geikie (1875), made little impression. The views of Lyell (1833), Ball (1863), J. W. Dawson (1864), Falconer (1864), Studer (1864), Murchison (1864, 1870), Ruskin (1865), Rutimeyer (1869), Whymper (1871), Bonney (1873), Pfaff (1874), Gurlt (1874), Judd (1876), prevailed, and the conclusions of Davis in 1882[72] fairly expressed the prevailing belief in Europe and in America:

''The amount of glacial erosion in the central districts has been very considerable, but not greatly in excess of pre-glacial soils and old talus and alluvial deposits. Most of the solid rock that was carried away came from ledges rather than from valleys; and glaciers had in general a smoothing rather than a roughening effect. In the outer areas on which the ice advanced it only rubbed down the projecting points; here it acted more frequently as a depositing than as an eroding agent.''

During the past quarter-century the cleavage in the ranks of geologists, brought about by Ramsay's classic paper, has remained. Fairchild and others in America, Heim, Bonney, and Garwood in Europe argue for insignificant erosion by glaciers; and Gannet, Davis, Gilbert, Tarr in America followed by Austrian workers present evidence for erosion on a gigantic scale. A perusal of the voluminous literature in the Journal and elsewhere shows that the difference of opinion is in part one of terms, the amount of erosion rather than the fact of erosion; it also arises from failure to differentiate the work of mountain glaciers and continental ice sheets, of Pleistocene glaciers and their present diminished representatives. The irrelevant contribution of physicists has also made for confusion.

It is interesting to note that the criteria for erosion of valleys by glaciers has long been established and by workers in different countries. Ramsay (1862) in

England outlined the problem and presented generalized evidence. Hector (1863) in New Zealand pointed out the significance of discordant drainage, the "hanging valleys" of Gilbert. The U-form, the broad lake-dotted floor, and the presence of cirques and the process of plucking were probably first described by LeConte (1873) in America. The truncation of valley spurs by glaciers pointed out by Studer in the Kerguelen Islands (1878) was used by Chamberlin (1883) as evidence of glacial scouring.

Conclusion.

During the past century many principles of land sculpture have emerged from the fog of intellectual speculation and unorganized observation and taken their place among generally accepted truths. Many of them are no longer subjects of controversy. Erosion has found its place as a major geologic agent and has given a new conception of natural scenery. Lofty mountains are no longer "ancient as the sun," they are youthful features in process of dissection; valleys and canyons are the work of streams and glaciers; fiords are erosion forms; waterfalls and lakes are features in process of elimination; many plains and plateaus owe their form and position to long-continued denudation. Modern landscapes are no longer viewed as original features or the product of a single agent acting at a particular time, but as ephemeral forms which owe their present appearance to their age and the particular forces at work upon them as well as to their original structure.

It is interesting to note the halting steps leading to the present viewpoint, to find that decades elapsed between the formulation of a theory or the recording of significant facts and their final acceptance or rejection, and to realize that the organization of principles and observations into a science of physiography has been the work of the present generation. Progress has been conditioned by a number of factors besides the intellectual ability of individual workers.

The influence of locality is plainly seen. Convincing evidence of river erosion was obtained in central France, the Pacific Islands, and the Colorado Plateau—regions

in which other causes were easily eliminated. Sculpture by glaciers passed beyond the theoretical stage when the simple forms of the Sierras and New Zealand Alps were described. The origin of loess was first discerned in a region where glacial phenomena did not obscure the vision. The complexity of the Glacial period asserted by geologists of the Middle West was denied by eastern students. The work of waves on the English coast impressed British geologists to such an extent that plains of denudation and inland valleys were ascribed to ocean work.

In the establishment of principles, the friendly interchange of ideas has yielded large returns. Many of the fundamental conceptions of earth sculpture have come from groups of men so situated as to facilitate criticism. It is impossible, even if desirable, to award individual credit to Venetz, Charpentier, and Agassiz in the formulation of the glacial theory; and the close association of Agassiz and Dana in New England and of Chamberlin and Irving in Wisconsin was undoubtedly helpful in establishing the theory of continental glaciation. From the intimate companionship in field and laboratory of Hutton, Playfair and Hope, arose the profound influence of the Edinburgh school, and the sympathetic coöperation of Powell, Gilbert, and Dutton has given to the world its classics in the genetic study of land forms.

The influence of ideas has been closely associated with clarity, conciseness, and attractiveness of presentation. Hutton is known through Playfair, Agassiz's contributions to glacial geology are known to every student, while Venetz, Charpentier, and Hugi are only names. Cuvier's discourses on dynamical geology were reprinted and translated into English and German, but Lamarck's "Hydrogéologie" is known only to book collectors. The verbose works of Guettard, although carrying the same message as Playfair's "Illustrations" and Desmarest's "Memoirs," are practically unknown, as is also Horace H. Hayden's treatise (1821) on the drift of eastern North America. It has been well said that the world-wide influence of American physiographic teaching is due in no small part to the masterly presentations of Gilbert and Davis.

It is surprising to note the delays, the backward steps, and the duplication of effort resulting from lack of familiarity with the work of the pioneers. Sabine says in 1864:[73]

"It often happens, not unnaturally, that those who are most occupied with the questions of the day in an advancing science retain but an imperfect recollection of the obligations due to those who laid the first foundations of our subsequent knowledge."

The product of intellectual effort appears to be conditioned by time of planting and character of soil as well as by quantity of seed. For example: Erosion by rivers was as clearly shown by Desmarest as by Dana and Newberry 50 years later. Criteria for the recognition of ancient fluviatile deposits were established by James Deane in 1847 in a study of the Connecticut Valley Triassic. Agassiz's proof that ice is an essential factor in the formation of till is substantially a duplication of Dobson's observations (1826).

The volumes of the Journal with their very large number of articles and reviews dealing with geology show that the interpretation of land forms as products of subaërial erosion began in France and French Switzerland during the later part of the eighteenth century as a phase of the intellectual emancipation following the Revolution. Scotland and England assumed the leadership for the first half of the nineteenth century, and the first 100 volumes of the Journal show the profound influence of English and French teaching. In America, independent thinking, early exercised by the few, became general with the establishment of the Federal survey, the increase in university departments, geological societies and periodicals, and has given to Americans the responsibilities of teachers.

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IV

A CENTURY OF GEOLOGY.—THE GROWTH OF KNOWLEDGE OF EARTH STRUCTURE

By JOSEPH BARRELL

Introduction

The Intellectual Viewpoint in 1818.

IN 1818, the year of the founding of the Journal, the natural sciences were still in their infancy in Europe. Geology was still subordinate to mineralogy, was hardly recognized as a distinct science, and consisted in little more than a description of the character and distribution of minerals and rocks. America was remote from the Old World centers of learning. The energy of the young nation was absorbed in its own expansion, and but a few of those who by aptitude were fitted to increase scientific knowledge were even conscious of the existence of such a field of endeavor. Under these circumstances the educative field open to a journal of science in the United States was an almost virgin soil. Original contributions could most readily be based upon the natural history of the New World, and the founder of the Journal showed insight appreciative of the situation in stating in the "Plan of the Work" in the introduction to the first volume that "It will be a leading object to illustrate AMERICAN NATURAL HISTORY, and especially our MINERALOGY and GEOLOGY.

At this time educated people were still satisfied that the whole knowledge of the origin and development of the earth so far as man could or should know it was embraced in the Book of Genesis. They were inclined to look with misgiving at attempts to directly interrogate the earth as to its history. Philosophers such as Descartes

and Liebnitz, the cosmogonists de Maillet and Buffon had been less instrumental in developing science than in fitting a few facts and many speculations to their systems of philosophy. By the opening of the nineteenth century, however, men of learning were coming to appreciate that the way to advance science was to experiment and observe, to collect facts and discourage unfounded speculation. Silliman's insight into the needs of geologic science is shown in the following quotation (1, pp. 6, 7, 1818):

"Our geology, also, presents a most interesting field of inquiry. A grand outline has recently been drawn by Mr. Maclure, with a masterly hand, and with a vast extent of personal observation and labour: but to fill up the detail, both observation and labour still more extensive are demanded; nor can the object be effected, till more good geologists are formed, and distributed over our extensive territory.

To account for the formation and changes of our globe, by excursions of the imagination, often splendid and imposing, but usually visionary, and almost always baseless, was, till within half a century, the business of geological speculations; but this research has now assumed a more sober character; the science of geology has been reared upon numerous and accurate observations of *facts;* and standing thus upon the basis of induction, it is entitled to a rank among those sciences which Lord Bacon's Philosophy has contributed to create. Geological researches are now prosecuted by actually exploring the structure and arrangement of districts, countries, and continents. The obliquity of the strata of most rocks, causing their edges to project in many places above the surface; their exposure, in other instances on the sides or tops of hills and mountains; or, in consequence of the intersection of their strata, by roads, canals, and river-courses, or by the wearing of the ocean; or their direct perforation, by the shafts of mines; all these causes, and others, afford extensive means of reading the interior structure of the globe.

The outlines of American geology appear to be particularly grand, simple, and instructive; and a knowledge of the important facts, and general principles of this science, is of vast practical use, as regards the interests of agriculture, and the research for useful minerals. Geological and mineralogical descriptions, and maps of particular states and districts, are very much needed in the United States; and to excite a spirit to furnish them will form one leading object of this Journal."

The Prolonged Influence of Outgrown Ideas.

Those interested in any branch of science should, as a matter of education, read the history of that special subject. A knowledge of the stages by which the present development has been attained is essential to give a proper perspective to the literature of each period. Much of the existing terminology is an inheritance from the first attempts at nomenclature, or may rest upon theories long discarded. Popular notions at variance with advanced teaching are often the forgotten inheritance of a past generation.

Gneiss, trap, and Old Red Sandstone are names which we owe to Werner. The "Tertiary period" and "drift" are relics of an early terminology. The geology of tourist circulars still speaks of canyons as made by "convulsions of nature." Popular writers still attribute to geologists a belief in a molten earth covered by a thin crust. Within the present century the eighteenth century speculations of Werner and his predecessors, postulating a supposed capacity of water to seep through the crust into the interior of the earth, resulting in a hypothetical progressive desiccation of the surface, views long abandoned by most modern geologists, have been revived by an astronomer into a theory of "planetology."

A review of the literature of a century brings to light certain tendencies in the growth of science. Each decade has witnessed a larger accumulation of observed facts and a fuller classification of these fundamental data, but the pendulum of interpretative theory swings away from the path of progress, now to one side, now to the other, testing out the proper direction. For decades the understanding of certain classes of facts may be actually retrogressive. A retrospect shows that certain minds, keen and unfettered by a prevailing theory, have in some directions been in advance of their generation. But the judgment of the times had not sufficient basis in knowledge for the separation and acceptance of their truer views from the contemporaneous tangle of false interpretations.

An interesting illustration of these statements regard-

ing the slow settling of opinion may be cited in regard to the significance of the dip of the Triassic formations of the eastern United States. The strata of the Massachusetts-Connecticut basin possess a monoclinal easterly dip which averages about 20 degrees to the east. Those of the New Jersey-Pennsylvania-Virginia basin possess a similar dip to the northwest. Both basins are cut by great faults and the dip is now accepted by practically all geologists as due to rotation of the crust blocks away from a geanticlinal axis between the two basins. Edward Hitchcock, whose work from the first shows an interpretative quality in advance of his time, states in 1823 (6, 74) regarding the dip of the Connecticut valley rocks:

"There is reason to believe that Mount Toby, the strata of which are almost horizontal, exhibits the original dip of these rocks, and that those cases in which they are more highly inclined are the result of some Plutonian convulsion. Such irregularity in the dip of coal fields is no uncommon occurrence."

In Hitchcock's Geology of Massachusetts, published in 1833, ten years later, geological structure sections of the Connecticut Valley rocks are given, the facts are discussed in detail and the dip ascribed to the elevatory forces. He says (l. c., pp. 213, 223):

"If it were possible to doubt that the new red sandstone formation was deposited from water, the surface of some of the layers of this shale would settle the question demonstrably. For it exhibits precisely those gentle undulations, which the loamy bottom of every river with a moderate current, presents. (No. 198.) But such a surface could never have been formed while the layers had that high inclination to the horizon, which many of them now present: so that we have here, also, decisive evidence that they have been elevated subsequently to their deposition. . . .

The objection of a writer in the American Journal of Science, that such a height of waters as would deposit Mount Toby, must have produced a lake nearly to the upper part of New Hampshire, in the Connecticut Valley, and thus have caused the same sandstone to be produced higher up that valley than Northfield, loses its force, when it is recollected that this formation was deposited before its strata were elevated. For the elevating force undoubtedly changed the relative level of different parts

Courtesy of *Popular Science Monthly.*

Edward Hitchcock

of the country. In this case, the disturbing force must have acted beneath the primary rocks. And besides, we have good evidence which will be shown by and by, that our new red sandstone was formed beneath the ocean. We cannot then reason on this subject from present levels."

In 1840, H. D. Rogers, a geologist who has acquired a more widely known name than Hitchcock, but who in reality showed an inferior ability in interpretation, made the following statements in explanation of the regional monoclinal dip of the New Jersey Triassic rocks averaging 15 to 20 degrees to the northwest:[1]

"Their materials give evidence of having been swept into this estuary, or great ancient river, from the south and southeast, by a current producing an almost universal dip of the beds towards the northwest, a feature clearly not caused by any uplifting agency, but assumed originally at the time of their deposition, in consequence of the setting of the current from the opposite or southeastern shore."

In 1842, at the third annual meeting of the Association of American Geologists both H. D. and W. B. Rogers argued (**43**, 170, 1842) against Sir Charles Lyell and E. Hitchcock that the present dip of the Triassic was the original slope of deposition, stating among other reasons that the footprints impressed upon the sediments often showed a slipping and a pushing of the soft clay in the direction of the downhill slope. In 1858 H. D. Rogers still held to the same views of original dip,[2] notwithstanding that a moderate amount of observation on the mud-cracked and rain-pitted layers would have supplied the proof that such must have dried as horizontal surfaces. The idea of inclined deposition is not yet wholly dead as it has been suggested more than once within the present generation as a means of escaping from the necessity of accepting the very great thicknesses of this and similar formations. Thus, as Brögger has remarked in another connection,—the ghosts of the old time stand ever ready to reappear.

In the present essay on the rise of structural geology as reflected through a century of publication in the Journal, attention will be given especially to two fields, that of structures connected with igneous rocks and that

of structures connected with mountain making, and emphasis will be placed upon the growth of understanding rather than upon the accumulating knowledge of details. The growth in both of these divisions of structural geology is well illustrated in the volumes of the Journal.

Structures and Relationships of Igneous Rocks.

Opposed Interpretations of Plutonists and Neptunists.

During the first quarter of the nineteenth century the geologic controversy between the Plutonists and Neptunists was at its height; the Plutonists, following the Scotchman, Hutton, holding to the igneous origin of basalt and granite, the Neptunists, after their German master, Werner of Freiberg, maintaining that these rocks had been precipitated from a primitive universal ocean. The Plutonists, although time has shown them to have been correct in all essential particulars, were for a generation submerged under the propaganda carried forward by the disciples of Werner. The "Illustrations of the Huttonian Theory of the Earth," a remarkable classic, worthy of being studied to-day as well as a century ago, was published in 1802 by John Playfair, professor of mathematics in the University of Edinburgh and a friend of Hutton, who had died five years previously. This volume was opposed by Robert Jameson, professor of natural philosophy in the same university, who had absorbed the ideas of the German school while at Freiberg and published in 1808 a volume on the "Elements of Geognosy," in which the philosophy of Werner is followed throughout and even obsidian and pumice are argued to be aqueous precipitates. The authority of the Wernerian autocracy caused its nomenclature to be adopted in the new world, but strong evidence against its interpretations was to be found in the actual structural relations displayed by the igneous rocks.

Contributions on Volcanic and Intrusive Rocks.

The accumulation and study of facts constituted the best cure for an erroneous theory. The publications of the Journal contributed toward this end by articles along

several lines. The most original contributions were those which dealt with the areal and structural geology of eastern North America, but equally valuable at that time for the broadening of scientific interest were the studies on the volcanic activities of the Hawaiian Islands, published through many years. Perhaps most valuable from the educative standpoint were the extensive republications in the Journal of the more important European researches, making them accessible to American readers. In volume **13** (1828), for example, a digest of Scrope's work on volcanoes is given, covering forty pages; and of Daubeny on active and extinct volcanoes, running over seventy-five pages and extending into vol **14.** Through these comprehensive studies the nature of volcanic action became generally understood during the first half of the nineteenth century and the original publications in the Journal were valuable in giving a knowledge of the activities of the Hawaiian volcanoes.

Early in the nineteenth century the whole of America still remained to be explored by the geologist. The regions adjacent to the centers of learning were among the first to receive attention and the Triassic basin of Connecticut and Massachusetts yielded information in regard to the nature of igneous intrusion. This basin, of unmetamorphic shales and sandstones, is occupied by the Connecticut River except at its southern end. The Formation contains within it sills, dikes, and outflows of basaltic rocks which because of their superior resistance to erosion constitute prominent hills, in places bounded by cliffs.

Silliman in 1806[3] described East Rock, New Haven, Connecticut, as a whinstone, trap, or basalt, and accounted for its presence on the supposition that it had

"actually been melted in the bowels of the earth and ejected among the superior strata by the force of subterraneous fire, but never erupted like lava, cooling under the pressure of the superincumbent strata and therefore compact or nonvesicular, its present form being due to erosion."

In these conclusions Silliman was correct. With but a limited amount of experience he was able to discriminate between the intrusive and effusive rocks and saw that the

prominence of this hill was due to the erosion of the sediments which once surrounded it.

An extensive paper on the geology of this region was published by Edward Hitchcock in 1823,[4] then just thirty years of age. This paper shows the evidence of extensive field observations, and his comments in regard to the trap and granite are of interest. Hitchcock gives five pages to the subject of "Greenstone Dykes in Old Red Sandstone" (6, 56-60, 1823) and makes the following statements:

"Professor Silliman conducted me to an interesting locality of these in East-Haven. They occur on the main road from New-Haven to East-Haven, less than half a mile from Tomlinson's bridge . . . (p. 56).

They are an interesting feature in our geology, and deserve more attention; and it is peculiarly fortunate that they should be situated so near a geological school and the first mineral cabinet in our country . . . (p. 58).

Origin of Greenstone.

Does the greenstone of the Connecticut afford evidence in favour of the Wernerian or of the Huttonian theory of its origin? Averse as I feel to taking a side in this controversy, I cannot but say, that the man who maintains, in its length and breadth, the original hypothesis of Werner in regard to the aqueous deposition of trap, will find it for his interest, if he wishes to keep clear of doubts, not to follow the example of D'Aubuisson, by going forth to examine the greenstone of this region, lest, like that geologist, he should be compelled, not only to abandon his theory, but to write a book against it. Indeed, when surveying particular portions of this rock, I have sometimes thought Bakewell did not much exaggerate when he said in regard to Werner's hypothesis, that, 'it is hardly possible for the human mind to invent a system more repugnant to existing facts.'

On the other hand, the Huttonian would doubtless have his heart gladdened, and his faith strengthened by a survey of the greater part of this rock. As he looked at the dikes of the old red sandstone, he would almost see the melted rock forcing its way through the fissures; and when he came to the amygdaloidal, especially to that variety which resembles lava, he might even be tempted to apply his thermometer to it, in the suspicion that it was not yet quite cool . . . (p. 59).

By treating the subject in this manner I mean no disrespect to any of the distinguished men who have adopted either side of

this question. To President Cooper especially, who regards the greenstone of the Connecticut as volcanic, I feel much indebted for the great mass of facts he has collected on the subject. And were I to adopt any hypothesis in regard to the origin of our greenstone, it would be one not much different from his'' (p. 60).

By 1833 and more clearly in 1841 Hitchcock had come to recognize the distinction between intrusive and extrusive basaltic sheets in the Connecticut valley. Dawson also came to regard the Acadian sheets as extrusive, and Emerson in 1882 recalled again the evidence for Massachusetts (**24**, 195, 1882). Davis, however, went a step further and by applying distinctive criteria not only separated intrusive and extrusive sheets throughout the whole Triassic area, but by using basalt flows as stratigraphic horizons unraveled for the first time the system of faults which cut the Triassic system. His preliminary paper (**24**, 345, 1882) was followed by many others.

From 1880 onward begins the period of precise structural field work. The older geologists mostly conceived their work after reconnaissance methods. From 1870 to 1880 a group of younger men entered geology who paid close attention to the solid geometry and mechanics of earth structures. In their hands physical and dynamical geology began to assume the standing of a precise and quantitative science. In the field of intrusive rocks the opening classic was by Gilbert, who in his volume on the geology of the Henry Mountains, published in 1880, made laccoliths known to the world. With the beginning of this new period we may well leave the subject of intrusive rocks and turn to the progress of knowledge in regard to those deeper and vaster bodies now known as batholiths. These, since erosion does not expose their bottoms, Daly separates from intrusives and classifies as subjacent. The batholiths consist typically of granite and granodiorite, and introduce us to the problem of granite.

Views on the Structural Relations of Granite.

Conscientious field observations were sufficient to establish the true nature of the intrusive and extrusive rocks. The case was very different, however, with the

nature and relations of the great bodies of granite, which may be taken in the structural sense as including all the visibly crystalline acidic and intermediate rocks, known more specifically as granite, syenite, and diorite.

The large bodies of granite, structurally classified as stocks, or batholiths, commonly show wedges, tongues, or dike networks cutting into the surrounding rocks. The relations, however, are not all so simple as this. Granites may cover vast areas, they are usually the older rocks, they are generally associated with regional metamorphism of the intruded formations, which metamorphism is now understood to be due chiefly to the heat and mineralizers given off from the granite magma, associated with mashing and shearing of the surrounding rocks. The granite was often injected in successive stages which alternated with the stages of regional mashing. A parallel or gneissic structure is thus developed which is in part due to mashing, in part to igneous injection. Where the ascent of heat into the cover is excessive, or where blocks are detached and involved in the magma, the latter may dissolve some of the older cover rocks, even where these were of sedimentary origin.

Thus between mashing, injection, and assimilation the genetic relationships of a batholith to its surroundings are in many instances obscure. Nevertheless, attention to the larger relations shows that the molten magma originated at great depths in the earth's crust, far below the bottoms of geosynclines, and consists of primary igneous material, not of fused sediments. From those depths it has ascended by various processes into the outer crust, where it crystallized into granite masses, to be later exposed by erosion. The amount of material which can be dissolved and assimilated must be small in comparison with the whole body of the magma. The original composition of the magma was probably basic, nearer that of a basalt than that of a granite. Differentiation of the molten mass is thought to cause the upper and lower parts of the chamber to become unlike, the lighter and more acidic portion giving rise to the great bodies of granite. With the exception of certain border zones the whole, however, is regarded as igneous rock risen from the depths.

The complex border relations, but more particularly certain academic hypotheses, led to a period of misunderstanding and retrogression in regard to the nature of granites. It constitutes an interesting illustration of the possibility of a wrong theory leading interpretation astray, chiefly through the magnification of minor into major factors. This history illustrates the dangers of qualitative science as compared to quantitative, of a single hypothesis as matched against the method of multiple working hypothesis. This flux of opinion in regard to the nature of granites may be traced through the volumes of the Journal.

E. Hitchcock in 1824 (**6**, 12) noted that in places granite appeared bedded, but in other places existed in veins which cut obliquely across the strata. Silliman, although careful not to deny the aqueous origin of some basalts, yet held that the field evidence of New England indicates for that region the igneous or Huttonian origin of trap and granite (**7**, 238, 1824).

In 1832 the following article by Hitchcock appeared in the Journal (**22**, 1, 70):

Report on the Geology of Massachusetts; examined under the direction of the Government of that State, during the years 1830 and 1831; by Edward Hitchcock, Prof. of Chemistry and Natural History in Amherst College.

A footnote adds that this is "published in this Journal by consent of the Government of Massachusetts, and intended to appear also in a separate form, and to be distributed among the members of the Legislature of the same State, about the time of its appearance in this work. It is, we believe, the first example in this country, of the geological survey of an entire State."

This article includes a geological map of the state and covers the subject of economic geology. The report brought forth the following remarks from a French reviewer in the *Revue Encyclopédique,* Aug. 1832, quoted in the Journal (**23**, 389, 1833):

"A single glance at this report, is sufficient to convince any one of the utility of such a work, to the state which has undertaken it; and to regret that there is so very small a part of the French territory, whose geological constitution is as well known to the public, as is now the state of Massachusetts. France has the greater cause to regret her being distanced in this race by

America, from her having a corps of mining engineers, who if they had the means, would, in a very short time furnish a work of the same kind, still more complete, of each of the departments."

The complete report published in 1833 is a work of 700 pages. Pages 465 to 517 are devoted to the subject of granite. Numerous detailed sketches are given showing contact relations. Nine pages are given to theoretical considerations and many lines of proof are given that granite is an igneous rock, molten from the internal heat of the earth, and intruded into the sedimentary strata. His statement is the clearest published in the world, so far as the writer is aware, up to that date, and marks Edward Hitchcock as one of the leading geologists of his generation in Europe as well as America. Unfortunately his views were largely lost to sight during the following generation.

In 1840 the first American edition of Mantell's Wonders of Geology gave currency to the idea that granite is proved to be of all geological ages up to the Tertiary (**39**, 6, 1840). In 1843 J. D. Dana pointed out (**45**, 104) that schistosity was no evidence of sedimentary origin. He regarded most granites as igneous as shown by their structural relations, but considers that some may have had a sedimentary origin.

Rise and Decline of the Metamorphic Theory of Granite.

Up to 1860 granite was regarded on the basis of the facts of the field as essentially an intrusive rock, but gneiss as a metamorphic product mostly of sedimentary origin. It seemed as though sound methods of research and interpretation were securely established. Nevertheless, a new era of speculation and a modified Wernerism arose at that time with a paper by T. Sterry Hunt, marking a retrogression in the theory of granite which lasted until his death in 1892.

In November, 1859, Hunt read before the Geological Society of London a paper on "Some Points in Chemical Geology" in which he announced that igneous rocks are in all cases simply fused and displaced sediments, the fusion taking place by the rise of the earth's internal

heat into deeply buried and water-soaked masses of sediments (see 30, 133, 1860). The germ of this idea of aqueo-igneous fusion was far older, due to Babbage and John Herschel, neither of them geologists, but such sweeping extensions of it had never before been published. Hunt had the advantage of a wide acquaintanceship with geological literature and chemistry. He wrote plausibly on chemical and theoretical geology, but his views were not controlled by careful field observations. In fact he wrote confidently on regions which apparently he had never seen and where a limited amount of field work would have shown him to have been fundamentally in error. A man of egotistical temperament. he sought to establish priority for himself in many subjects and in order to cover the field made many poorly founded assertions. Building on to another Wernerian idea, he held that many metamorphic minerals had a chronologic value comparable to fossils—staurolite for example indicating a pre-Silurian age—and on this basis divided the crystalline rocks into five series. Although there is much of value buried in Hunt's work it is difficult to disentangle it, with the result that his writings were a disservice to the science of geology. Although carrying much weight in his lifetime, they have passed with his death nearly into oblivion.

Marcou, with a limited knowledge of American geology, and but little respect for the opinions of others, had published a geologic map of the United States containing gross errors. In support of his views he read in November, 1861, a paper on the Taconic and Lower Silurian Rocks of Vermont and Canada. In the following year he was severely reviewed by "T," who states positively in controverting Marcou (33, 282, 283, 1862) that "the granites (of the Green Mountains) are evidently strata altered in place."

"Mr. Marcou should further be informed that the granites of the Alpine summits, instead of being, as was once supposed, eruptive rocks, are now known to be altered strata of newer Secondary and Tertiary age. A simple structure holds good in the British Islands, where as Sir Roderick Murchison has shown in his recent Geological map of Scotland, Ben Nevis and Ben Lawers are found to be composed of higher strata, lying in

synclinals. This great law of mountain structure would alone lead us to suppose that the gneiss of the Green mountains, instead of being at the base, is really at the summit of the series. . . .

We cannot here stop to discuss Mr. Marcou's remark about 'the unstratified and oldest crystalline rocks of the White mountains' which he places beneath the lower Taconic series. Mr. Lesley has shown that these granites are stratified, and with Mr. Hunt, regards them as of Devonian Age. (This Journal, vol. **31**, p. 403.) Mr. Marcou has come among us with notions of mountains upheaved by intrusive granites, and similar antiquated traditions, now, happily for science, well nigh forgotten."

It is seen that Marcou, notwithstanding the general character of his work, happened to be nearer right in some matters than were his critics, and that "T" had adopted to the limit the views of Hunt.

The recovery of geology from this period of confusion was partly owing to the slow accumulation of opposed facts; especially to a recognition of the fact that the overplaced relation of the granite gneisses in western Scotland was due to great overthrusts; also to the evidence of the clearly intrusive nature of many of the Cordilleran granites. The recovery of a sounder theory was hastened, however, by the application of criticisms by J. D. Dana in the Journal. In 1866 (**42**, 252) Dana pointed out that sedimentary rocks in Pennsylvania, in Nova Scotia, and other regions which had been buried to a depth of at least 16,000 feet are not metamorphic. Mere depth of burial of sediments was not sufficient therefore to produce metamorphism and aqueo-igneous fusion. The baseless and speculative character of the use of minerals as an index of age and of Hunt's interpretation of New England geology in general was shown by Dana in 1872 (**3**, 91). The following year Dana pointed out clearly that igneous eruptions in general have been derived from a deep-seated source and did not come from the aqueo-igneous fusion of sediments. As to gradations between true igneous rocks and fused and displaced sediments he makes the following statements (**6**, 114, 1873):

"Again, the plastic rock-material that may be derived from the fusion or semifusion of the supercrust, (that is, of rocks

originally of sedimentary origin,) gives rise to "igneous" rocks often not distinguishable from other igneous rocks, when it is ejected through fissures far from its place of origin; while crystalline rocks are simply *metamorphic* if they remain in their original relations to the associated rocks, or nearly so.

Between these latter igneous rocks and the metamorphic there may be indefinite gradations, as claimed by Hunt. But if our reasonings are right, the great part of igneous rocks can be proved to have had no such supercrust origin. The argument from the presence of moisture or of hydrous minerals in such rocks in favor of their origin from the fusion of sediments has been shown to be invalid."

The injected marginal rocks and the post-intrusive metamorphism of most of the New England granites has, however, obscured more or less their real igneous nature so that the gradation from metamorphic sediments through igneous gneisses to granites could be read in either direction. These features misled Dana who accepted the prevailing idea of the general metamorphic origin of granite. Dana makes the following statement (6, 164, 1873):

"But Hunt is right in holding that in general granite and syenite (the quartz-bearing syenite) are undoubtedly metamorphic rocks where not vein-formations, as I know from the study of many examples of them in New England; and the veins are results of infiltration through heated moisture from the rocks adjoining some part of the opened fissures they fill."

Granite, although regarded at this time as the extreme of the metamorphic series and originating from sediments, was looked upon as typically Archean in age, though in some cases younger. Such a doctrine permitted such extreme misinterpretations as that of Clarence King and S. F. Emmons on the nature of the intrusive granite of the Little Cottonwood canyon in the Wahsatch Range. This body cuts across 30,000 feet of Paleozoic rocks and to the careful observer, as later admitted by Emmons, shows clear evidence of its transgressive nature. But at that time it was generally considered that granite mountains were capable of resisting the erosion of all geological time. Consequently it did not seem incredible to King and his associates that here a great granite range of Archean origin had stood

up through Paleozoic time until gradual subsidence had permitted it to be buried beneath 30,000 feet of sediments.[5]

It may seem to the present day reader that such a misinterpretation, doing violence to fundamental geologic knowledge as now recognized, was inexcusable; but in the light of the history of geology as here detailed it is seen to have been the interpretation natural to that time. It is true that a careful examination of the facts of that very field would have proved the post-Paleozoic and intrusive nature of that great granite body now known as the Little Cottonwood batholith, but Emmons has explained the rapid and partial nature of the observations which they were compelled to make in order to keep up to their schedule of progress (**16**, 139, 1903).

Whitney had found some years earlier that the granites of the Sierra Nevada were igneous rocks intrusive into the Triassic and Jurassic strata. The Lake Superior geologists began to show in the eighties that granite was there an intrusive igneous rock. R. D. Irving and Wadsworth noted these relations. Lawson in 1887 pointed out emphatically (**33**, 473) that the granites of the Rainy Lake region, although basal, were younger than the schists which lay above them. The granite-gneisses he held were of clearly the same igneous origin as the granites and neither gave any field evidence of being fused and displaced sediments. From this time forward the truly igneous nature of granite became increasingly accepted until now the notion of its being made of sedimentary rocks softened and recrystallized by the rise of the isogeotherms through deep burial is as obsolete as the still older doctrine of the Neptunists that granite was laid down as a crystalline precipitate on the floor of the primitive ocean.

The recognition of the truly igneous nature of granites has been followed in the present generation by a series of studies on their structural relations and mode of genesis. A number of important initial articles on various aspects of structure and contact relations have appeared in the Journal, but this sketch of the history of the subject may well stop with the introduction to this modern period.

Orogenic Structures.

Views of Plutonists and Neptunists.

Orogenic structures are, as the name implies, those connected with the birth of mountains. Nearly synonymous terms are deformative or secondary structures. On a small scale this division embraces the phenomena exposed in the rock ledge or quarry face, or in the dips and dislocations varying from one exposure to another. These structures include faults, folds, and foliation. On a larger scale are included the relations of the different ranges of a mountain system to each other, relations to previous geologic history, relations to the earth as a whole, and to the forces which have generated the structures.

In order to see the stage of development of this subject in 1818 and its progress as reflected through the publications of a century, more particularly in the Journal, it is desirable to turn again to those two treatises emanating from Edinburgh at the beginning of the nineteenth century and representing two opposite schools of thought, the Plutonists and Neptunists.

Playfair, in 1802, devotes nineteen pages to the subject of the inflection and elevation of strata.[6] He places emphasis on the characteristic parallelism of the strike of the folds throughout a region, as shown through the intersection of the folds by a horizontal plane of erosion. He contrasts this with the arches shown in a transverse section and enlarges on our ability to study the deeply buried strata through the denudation of the folded structure. He argues from these relations that the structures can not be explained by the vague appeal of the Neptunists to forces of crystallization, to slopes of original deposition, or to sinking in of the roofs of caverns. The causes he argues were heat combined with pressure. As to the directions in which the pressure acted he is not altogether clear, but apparently regards the pressure as acting in upward thrusts against the sedimentary planes, the latter yielding as warped surfaces. His method of presentation is that of inductive reasoning from facts, but he stopped short of the conception of horizontal compression through terrestrial contraction.

Jameson, professor of natural history in the same university, in 1808 contemptuously ignores the work of Hutton and Playfair in what he calls the "*monstrosities* known under the name of Theories of the Earth." In a couple of pages he confuses and dismisses the whole subject of deformation. He states:[7]

"It is therefore a fact, that all inclined strata, with a very few exceptions, have been formed so originally, and do not owe their inclination to a subsequent change.

When we examine the structure of a mountain, we must be careful that our observations be not too micrological, otherwise we shall undoubtedly fail in acquiring a distinct conception of it. This will appear evident when we reflect that the geognostic features of Nature are almost all on the great scale. In no case is this rule to be more strictly followed than in the examination of the stratified structure.

By not attending to this mode of examination, geognosts have fallen into numberless errors, and have frequently given to extensive tracts of country a most irregular and confused structure. Speculators building on these errors have represented the whole crust of the globe as an irregular and unseemly mass. It is indeed surprising, that men possessed of any knowledge of the beautiful harmony that prevails in the structure of organic beings could for a moment believe it possible, that the great fabric of the globe itself,—that magnificent display of Omnipotence,—should be destitute of all regularity in its structure, and be nothing more than a heap of ruins."

This was the attitude of a leader of British opinion toward the subject of deformational geology from which the infant science had to recover before progress could be made. The early maps were essentially mineralogical and lithological. The order of superposition and the consequent sequence of age was regarded as settled by Werner in Germany and not requiring investigation in America. The early examples of structure were sections drawn with exaggerated vertical scales and those of Maclure do not show detail.

Recognition of Appalachian Structures.

Following the founding of the Journal in 1818 there is observable a growth in the quality and detail of geological mapping. Dr. Aiken, professor of natural philosophy

and chemistry in Mt. St. Mary's College, published in the Journal in 1834 (**26**, 219) a vertical section extending between Baltimore and Wheeling, a distance of nearly 250 miles, on a scale of about 7 miles per inch. The succession of rocks is carefully shown and the direction of dip, but no attempt is made to show the underground relations, the stratigraphic sequence, and the folded structures which are so clear in that Appalachian section. The text also shows that the author had not recognized the folded structure. Furthermore, where the folds cease at the Alleghany mountain front, the flat strata are shown as resting unconformably on the folded rocks to the east.

R. C. Taylor, geologist, civil and mining engineer, was from 1830 to 1835 the leading student of Pennsylvanian geology as shown by the publication in 1835 of four papers aggregating over 80 pages in the Transactions of the Geological Society of Pennsylvania. His work is noticeable for accuracy in detail and no doubt was influential in setting a high standard for the state geological survey which immediately followed.

H. D. and W. B. Rogers have been given credit in this country, and in Europe also, as being the leading expounders of Appalachian structure. Merrill speaks of H. D. Rogers as unquestionably the leading structural geologist of his time.[8] To the writer, this attributed position appears to be due to his opportunities rather than to scientific acumen. The magnificent but readily decipherable folded structure of Pennsylvania, the relationships of coal and iron to this structure, the considerable sums of money appropriated, and the work of a corps of able assistants were factors which made it comparatively easy to reach important results. In ability to weigh facts and interpret them Edward Hitchcock showed much more insight than H. D. Rogers, while in the philosophic and comprehensive aspects of the subject J. D. Dana far outranks him.

H. D. Rogers in his first report on the geological survey of New Jersey, 1836, recognizes that the Cambro-Silurian limestones (lower Secondary limestones) were deposited as nearly horizontal beds and the ridges of pre-Cambrian gneiss (Primary) had been pushed up as

anticlinal axes (p. 128). He also clearly recognized the distinction between slaty cleavage and true dip as shown in the Ordovician slates (p. 97). Between 1836 and 1840 he had learned a great deal on the nature of folds as is shown in his Pennsylvania report for 1839 and the structure sections in his New Jersey report for 1840.

R. C. Taylor, who had now become president of the board of directors of the Dauphin and Susquehanna Coal Company, published in the Journal in 1841 (**41**, 80) an important paper entitled "Notice of a Model of the Western portion of the Schuylkill or Southern Coal Field of Pennsylvania, in illustration of an Address to the Association of American Geologists, on the most appropriate modes for representing Geological Phenomena." In this paper he calls attention to the value of modeling as a means of showing true relations in three dimensions. He condemns the custom prevalent among geologists of showing structure sections with an exaggerated vertical scale with its resultant topographic and structural distortions. Taylor was widely acquainted with the structure of Pennsylvania, Maryland, and Virginia.

Nature of Forces Producing Folding.

In 1825 Dr. J. H. Steele sent to Professor Silliman two detailed drawings and description of an overturned fold at Saratoga Lake, New York. As to the significance of this feature Steele makes the following statement (**9**, 3, 1825):

"It is impossible to examine this locality without being strongly impressed with the belief that the position which the strata here assume could not have been effected in any other way than by a power operating from beneath upwards and at the same time possessing a progressive force; something analogous to what takes place in the breaking up of the ice of large rivers. The continued swelling of the stream first overcomes the resistance of its frozen surface and having elevated it to a certain extent, it is forced into a vertical position, or thrown over upon the unbroken stratum behind, by the progressive power of the current."

So far as the present writer is aware this is the first recognition in geological literature of the evidence of a

horizontally compressive and overturning force as a cause of folding.

To E. Hitchcock belongs the credit of being the first to describe overturning and inversion of strata on a large scale, but without clearly recognizing it as such. In western Massachusetts metamorphism is extreme in the lower Paleozoic rocks in the vicinity of the overthrust mass of Archean granite-gneiss which constitutes the Hoosic range. The Paleozoic rocks of the valley to the west are overturned and appear to dip beneath the older rocks. Farther west the metamorphism fades out and the series assumes a normal position. Such an inverted relation, up to that time unknown, is described in 1833 as follows by Hitchcock in his Geology of Massachussetts (pp. 297, 298):

"But a singular anomaly in the superposition of the series of rocks above described, presents a great difficulty in this case. The strata of these rocks almost uniformly dip to the east: that is, the newer rocks seem to crop out beneath the older ones; so that the saccharine limestone, associated with gneiss in the eastern part of the range, seems to occupy the uppermost place in the series. Now as superposition is of more value in determining the relative ages of rocks than their mineral characters, must we not conclude that the rocks, as we go westerly from Hoosac mountain, do in fact belong to older groups? The petrifactions which some of them contain, and their decidedly fragmentary character, will not allow such a supposition to be indulged for a moment. It is impossible for a geologist to mistake the evidence, which he sees at almost every step, that he is passing from older to newer formations, just as soon as he begins to cross the valley of Berkshire towards the west. We are driven then to the alternative of supposing, either that there must be a deception in the apparent outcrop of the newer rocks from beneath the older, or that the whole series of strata has been actually thrown over, so as to bring the newest rocks at the bottom. The latter supposition is so improbable that I cannot at present admit it."

Hitchcock tried to reconcile the evidence by a series of unconformities and inclined deposition, but finds the solution unsatisfactory.

In this same year, 1833, Elie de Beaumont, a distinguished French geologist, published his theory of the origin of mountains. He advanced the idea that since

the globe was cooling it was condensing, and the crust, already cool, must suffer compression in adjusting itself to the shrinking molten interior. He concluded from the evidence shown in Europe that the collapse of the crust occurred violently and rapidly at widely spaced intervals of time. This hypothesis introduced the idea of mountain folding by horizontal compressive forces. The theoretical paper of de Beaumont, together with further observations by Hitchcock and others, led the latter in 1841 to a final belief in the inversion of strata on a large scale by horizontal compression. His conclusions are expressed in an important paper published in the Journal (**41**, 268, 1841) and given on April 8, 1841, as the First Anniversary Presidential Address before the Association of American Geologists. This comprehensive summary of American geology occupies 43 pages. Three pages are given to the inverted structure of the Appalachians from which the following paragraphs may be quoted:

"We have all read of the enormous dislocations and inversions of the strata of the Alps; and similar phenomena are said to exist in the Andes. Will it be believed, that we have an example in the United States on a still more magnificent scale than any yet described? . . .

Let us suppose the strata between Hudson and Connecticut rivers, while yet in the plastic state, (and the supposition may be extended to any other section across this belt of country from Canada to Alabama,) and while only slightly elevated, were acted upon by a force at the two rivers, exerted in opposite directions. If powerful enough, it might cause them to fold up into several ridges; and if more powerful along the western than the eastern side, they might fall over so as to take an inverted dip, without producing any remarkable dislocations, while subsequent denudation would give to the surface its present outline. . . .

Fourthly, we should readily admit that such a plication and inversion of the strata might take place on a small scale. If for instance, we were to press against the extremities of a series of plastic layers two feet long, they could easily be made to assume the position into which the rocks under consideration are thrown. Why then should we not be equally ready to admit that this might as easily be done, over a breadth of fifty miles, and a length of twelve hundred, provided we can find in nature, forces

sufficiently powerful? Finally, such forces do exist in nature, and have often been in operation.''

The advanced nature of these conceptions may be appreciated by contrasting them with those put forth by H. D. and W. B. Rogers on April 29, 1842, before the third annual meeting of the same body (**43**, 177, 1842) and repeated by them before the British Association at Manchester two months later. In their own words, the Rogers brothers from their studies on the folds shown in Pennsylvania and Virginia, conceived mountain folds in general to be produced by much elastic vapor escaping through many parallel fissures formed in succession, producing violent propulsive wave oscillations on the surface of the fluid earth beneath a thin crust. Thus actual billows are assumed to have rolled along through the crust. They did not think tangential pressure alone could produce folds. Such pressures were regarded as secondary, produced by the propagation of the waves and the only expression of tangential forces which they admitted was to fix the folds and hold them in position after the violent oscillation had subsided (**44**, 360, 1843). The leading British geologists De la Beche and Sedgwick criticized adversely this remarkable theory, stating that they could see no such analogy in mountain folds to violent earthquake waves and that in their opinion the slow application of tangential force was sufficient to account for the phenomena (**44**, 362-365, 1843).

H. D. Rogers in the prosecution of the geological survey of Pennsylvania displayed notable organizing ability and persistence in accomplishment, even to advancing personally considerable sums of money, trusting to the state legislature to later reimburse him. Finally, after many delays by the state, the publication was placed directly in his charge and he produced in 1858 a magnificent quarto work of over 1,600 pages, handsomely illustrated, and accompanied by an atlas. It is excellent from the descriptive standpoint, standing in the first class. Measured as a contribution to the theory of dynamical geology, the explanatory portions were, however, thirty years behind the times. The same hypotheses are put forth in 1858 as in 1842. There is no acceptance of the views

of Lyell concerning the uniformitarian principles expounded by this British leader in 1830, or of the nature of orogenic forces as published by Elie de Beaumont in 1833. Rogers rejects the view that cleavage is due to compression and suggests "that both cleavage and foliation are due to the parallel transmission of planes or waves of heat, awakening the molecular forces, and determining their direction.[9] Thus a mere maze of words takes the place of inductive demonstrations already published.

In following the play of these opposing currents of geologic thought we reach now the point where a period of brilliant progress in the knowledge of mountains and of continental structures begins in the work of J. D. Dana. In 1842 Dana returned from the Wilkes Exploring Expedition and the following year began the publication of the series of papers which for the next half century marked him as the leader in geologic theory in America. His work is of course to be judged against the background of his times. His papers mark distinct advances in many lines and are characterized throughout by breadth of conception and especially by clear and logical thinking. His work was published very largely in the Journal, of which after a few years he became chief editor. His first contribution on the subject of mountain structures, entitled "Geological results of the earth's contraction in consequence of cooling," was published in 1847 (**3**, 176). The evidence of horizontal pressure was first perceived in France as shown by the features of the Alps. Elie de Beaumont connected it, by means of the theory of a cooling and contracting globe, with the other large fact of the increase of temperature with descent in the crust. Dana credits the Rogers brothers with first making known the folded structures of the Appalachians, but objects to their interpretation of origin. He showed by means of diagrams that the folds are to be explained by lateral pressure, the direction of overturning indicating the direction from which the driving force proceeded.

The Rogers brothers and especially James Hall, in working out the Appalachian stratigraphy, had noted that the formations, although accumulating to a maximum thickness of between 30,000 and 40,000 feet, showed

evidences that the successive formations were deposited in shallow water. It suggested to them that the weight of the accumulating sediments was the cause of subsidence, each foot of sediment causing a foot of down sinking. This idea has continued to run through various text books in geology for half a century, yet Dana early saw the fallacy and in 1863 in the first edition of his Manual of Geology (p. 717) states "whether this is an actual cause or not in geological dynamics is questionable." In 1866 in an important article on "Observations on the origins of some of the earth's features," Dana deals more fully and finally with this subject (**42**, 205, 252, 1866). He shows that such an effect of accumulating sediment postulates a delicate balance, a very thin crust and no resistance below. If such a weakness were granted it would be impossible for the earth to hold up mountains. Furthermore such subsidence was not regular during its progress and finally in the long course of geologic time gave place to a reverse movement of elevation.

Hall had pointed out the fact that the sediments were thickest on the east in the region of mountain folding and thinned out to a fraction of this thickness in the broad Mississippi basin. Hall argued that the mere subsidence of the trough would produce the observed folding and that the folding was unrelated to mountain making or crustal shortening. In supposed proof he cited the fact that the Catskills consist of unfolded rock, are higher than the folded region to the south, and nearly as high as the highest metamorphic mountains to the east.[10] Hall and all his contemporaries were handicapped in their geological theories by a complete inappreciation of the importance of subaërial denudation. For subscribing to these errors of their time even the ablest men should not be held responsible. Hall was the most forcible personality in geology in his generation. His contributions to paleontology were superb. His perception of the relation existing between troughs of thick sediments and folded structures was a contribution of the first importance; yet in the structural field his argument as to the production of the Appalachian folds by mere subsidence during deposition indicates a remarkable inability to

apply the logical consequences of his hypothesis to the nature of the folds as already made known by the Rogers. Dana pointed out in reply to Hall that the folding did not correspond to the requirements of Hall's hypothesis, especially as the folding took place not during, but after the close of the vast Paleozoic deposition. Dana states in conclusion on Hall's hypothesis (**42**, 209, 1866) that "It is a theory of the origin of mountains with the origin of mountains left out."

The Theory of Geosynclines and Geanticlines.

The fact that systems of folded strata lie along axes of especially thick sediments and that this implied subsidence during deposition was Hall's contribution to geologic theory, but curiously enough he failed, as shown, to connect it with the subsequent nature of mountain folding. He did not see why such troughs should be weak to resist horizontal compression. The clear recognition of this relationship was the contribution of Le Conte, who in a paper on "A theory of the formation of the great features of the earth's surface" (**4**, 345, 460, 1872), reached the conclusion that "mountain chains are formed by the mashing together and the up-swelling of sea bottoms where immense thicknesses of sediment have accumulated."

As to the cause why mashing should take place along troughs of thick sediments Le Conte adopts the hypothesis of aqueo-igneous fusion proposed independently long before by Babbage and Herschel and elaborated into a theory of igneous rocks by Hunt. Under this view, as the older sediments became deeply buried, the heat of the earth's interior ascended into them, and since they included the water of sedimentation a softening and metamorphism resulted. Dana had shown, however, six years previously (**42**, 252, 1866), as the following quotation will indicate, that metamorphism of sediments required more than deep burial and that no such weakening as was postulated by Herschel had occurred:

"The correctness of Herschel's principle cannot be doubted. But the question of its actual agency in ordinary metamorphism must be decided by an appeal to facts; and on this point I would here present a few facts for consideration.

The numbers and boldness of the flexures in the rocks of most metamorphic regions have always seemed to me to bear against the view that the heat causing the change had ascended by the very quiet method recognized in this theory. . . .

But there are other facts indicating a limited sufficiency to this means of metamorphism. These are afforded by the great faults and sections of strata open to examination. In the Appalachian region, both of Virginia and Pennsylvania, faults occur, as described by the Professors Rogers, and by Mr. J. P. Lesley, which afford us important data for conclusions. Mr. Lesley, an excellent geologist and geological observer, who has explored personally the regions referred to, states that at the great fault of Juniata and Blair Cos., Pennsylvania, the rocks of the Trenton period are brought up to a level with those of the Chemung, making a dislocation of at least 16,000, and probably of 20,000, feet. And yet the Trenton limestone and Hudson River shales are not metamorphic. Some local cases of alteration occur there, including patches of roofing slate; but the greater part of the shales are no harder than the ordinary shales of the Pennsylvania Coal formation.

At a depth of 16,000 feet the temperature of the earth's crust, allowing an increase of 1° F. for 60 feet of descent, would be about 330° F.; or with 1° F. for 50 feet, about 380° F.—either of which temperatures is far above the boiling point of water; and with the thinner crust of Paleozoic time the temperature at this depth should have been still higher. But, notwithstanding this heat, and also the compression from so great an overlying mass, the limestones and shales are not crystalline. The change of parts of the shale to roofing slate is no evidence in favor of the efficiency of the alleged cause; for such a cause should act uniformly over great areas."

The next contribution to the theory of orogeny was a series of papers published in 1873 by Dana, entitled "On some results of the earth's contraction from cooling, including a discussion on the origin of mountains and the nature of the earth's interior."[11] This contribution, viewed as a whole, ranks among the first half dozen papers on the science of mountains. The following quoted paragraphs give a view of the scope of this article:

"Kinds and Structure of Mountains."

"While mountains and mountain chains all over the world, and low lands, also, have undergone uplifts, in the course of their long history, that are not explained on the idea that all

mountain elevating is simply what may come from plication or crushing, the *component parts* of mountain chains, or those simple mountains or mountain ranges that are the product of one *process of making*—may have received, *at the time of their original making,* no elevation beyond that resulting from plication.

This leads us to a grand distinction in orography, hitherto neglected, which is fundamental and of the highest interest in dynamical geology; a distinction between—

1. A simple or *individual* mountain mass or range, which is the result of *one process of making,* like an individual in any process of evolution, and which may be distinguished as a *monogenetic* range, being *one in genesis;* and

2. A composite or *polygenetic* range or chain, made up of two or more monogenetic ranges combined.

The Appalachian chain—the mountain region along the Atlantic border of North America—is a *polygenetic* chain; it consists, like the Rocky and other mountain chains, of several *monogenetic* ranges, the more important of which are: 1. The Highland range (including the Blue Ridge or parts of it, and the Adirondacks also, if these belong to the same process of making) pre-Silurian in formation; 2. The Green Mountain range, in western New England and eastern New York, completed essentially after the Lower Silurian era or during its closing period; 3. The Alleghany range, extending from southern New York southwestward to Alabama, and completed immediately after the Carboniferous age.

The making of the Alleghany range was carried forward at first through a long-continued subsidence—a *geosynclinal* (not a *true* synclinal, since the rocks of the bending crust may have had in them many true or simple synclinals as well as anticlinals), and a consequent accumulation of sediments, which occupied the whole of Paleozoic time; and it was completed, finally, in great breakings, faultings and foldings or plications of the strata, along with other results of disturbance.

These examples exhibit the characteristics of a large class of mountain masses or ranges. A geosynclinal accompanied by sedimentary depositions, and ending in a catastrophe of plications and solidification, are the essential steps, while metamorphism and igneous ejections are incidental results. The process is one that produces final stability in the mass and its annexation generally to the more stable part of the continent, though not stable against future oscillations of level of *wider range,* nor against denudation.

It is apparent that in such a process of formation elevation by direct uplift of the underlying crust has no necessary place. The attending plications may make elevations on a vast scale

and so also may the shoves upward along the lines of fracture, and crushing may sometimes add to the effect; but elevation from an upward movement of the downward bent crust is only an incidental concomitant, if it occur at all.

We perceive thus where the truth lies in Professor Le Conte's important principle. It should have in view alone *monogenetic* mountains and these only *at the time of their making.* It will then read, plication and shovings along fractures being made more prominent than crushing:

Plication, shoving along fractures and crushing are the true sources of the elevation that takes place *during the making* of geosynclinal monogenetic mountains.

And the statement of Professor Hall may be made right if we recognize the same distinction, and, also, reverse the order and causal relation of the two events, accumulation and subsidence; and so make it read:

Regions of monogenetic mountains were, previous, and preparatory, to the making of the mountains, areas each of a slowly progressing geosynclinal, and, *consequently,* of thick accumulations of sediments.

The prominence and importance in orography of the mountain individualities described above as originating through a geosynclinal make it desirable that they should have a distinctive name; and I therefore propose to call a mountain range of this kind a *synclinorium,* from *synclinal* and the Greek ὄρος, mountain.

This brings us to another important distinction in orographic geology—that of a second kind of monogenetic mountain. The *synclinoria* were *made through a progressing geosynclinal.* Those of the second kind, here referred to, were *produced by a progressing geanticlinal.* They are simply the upward bendings in the oscillations of the earth's crust—the geanticlinal waves, and hardly require a special name. Yet, if one is desired, the term *anticlinorium,* the correlate of *synclinorium,* would be appropriate. Many of them have disappeared in the course of the oscillations; and yet, some may have been for a time—perhaps millions of years—respectable mountains.

The geosynclinal ranges or synclinoria have experienced in almost all cases, since their completion, true elevation through great geanticlinal movements, but movements that embraced a wider range of crust than that concerned in the preceding geosynclinal movements, indeed a range of crust that comes strictly under the designation of a polygenetic mass."

"The Condition of the Earth's Interior."

"The condition of the earth's interior is not among the geological results of contraction from cooling. But these results

offer an argument of great weight respecting the earth's interior condition, and make it desirable that the subject should be discussed in this connection. Moreover, the facts throw additional light on the preceding topic—the origin of mountains.

It seems now to be demonstrated by astronomical and physical arguments—arguments that are independent, it should be noted, of direct geological observation—that the interior of our globe is essentially solid. But the great oscillations of the earth's surface, which have seemed to demand for explanation a liquid interior, still remain facts, and present apparently a greater difficulty than ever to the geologist. Professor Le Conte's views, in volume iv, were offered by him as a method of meeting this difficulty; yet, as he admits in his concluding remarks, the oscillations over the interior of a continent, and the fact of the greater movements on the borders of the larger ocean, were left by him unexplained. Yet these oscillations are not more real than the changes of level or greater oscillations which occurred along the sea border, where mountains were the final result; and this being a demonstrated truth, no less than the general solidity of the earth's interior, the question comes up, how are the two truths compatible?

The geological argument on the subject (the only one within our present purpose) has often been presented. But it derives new force and gives clearer revelations when the facts are viewed in the light of the principles that have been explained in the preceding part of this memoir.

The Appalachian subsidence in the Alleghany region of 35,000 to 40,000 feet, going on through all the Paleozoic era, was due, as has been shown, to an actual sinking of the earth's crust through lateral pressure, and not to local contraction in the strata themselves or the terranes underneath. But such a subsidence is not possible, unless seven miles—that is, seven miles in maximum depth and over a hundred in total breadth—unless seven miles of *something* were removed, in its progress, from the region beneath.

If the matter beneath was not aërial, then liquid or viscous rock was pushed aside. This being a fact, it would follow that there existed, underneath a crust of unascertained thickness, a sea or lake of mobile (viscous or plastic) rock, as large as the sinking region; and also that this great viscous sea continued in existence through the whole period of subsidence, or, in the case of the Alleghany region, through all Paleozoic time—an era estimated on a previous page to cover at least thirty-five millions of years, if time since the Silurian age began embraced fifty millions of years.

The facts thus sustain the statement that lateral pressure

produced not only the subsidence of the Appalachian region through the Paleozoic, but also, cotemporaneously, and as its essential prerequisite, the rising of a sea-border elevation, or geanticlinal, parallel with it; and that both movements demanded the existence beneath of a great sea of mobile rock."

The recognition of regional *warping* as a major factor in the larger structure of mountain systems, and the expression of that factor in the terms geosyncline and geanticline forms a notable advance in geologic thought. Subsequent folding on a regional scale results in the development of synclinoria and anticlinoria. Van Hise has given these latter terms wide currency, but apparently inadvertently has used synclinorium in a different sense than that in which Dana defined it. Dana gave the word to a mountain range made by the mashing and uplift of a geosyncline, Van Hise defines it as a downfold of a large order of magnitude, embracing anticlines and synclines within it; anticlinorium he uses for a corresponding up fold.[12] Rice has called attention to this change of definition,[13] but Van Hise's usage is likely to prevail, since they are needed terms for the larger mountain structure and do not require a determination of the previous limits of upwarp and downwarp,—of original denudation and deposition. Furthermore, a geosyncline in mountain folding may have one side uplifted, the other side depressed and there are reasons for regarding the folds of Pennsylvania, Dana's type synclinorium, as representing but the western and downfolded side of the Paleozoic geosyncline. Under that view the folded Appalachians of Pennsylvania constitute a synclinorium in both the sense of Dana and Van Hise.

The Ultimate Cause of Crustal Compression.

The next important advance in the theory of mountains was made by C. E. Dutton who in 1874 published in the Journal (**8**, 113-123) an article entitled "A criticism upon the contractional hypothesis." Dutton gives reasons for holding that the amount of folding and shortening exhibited in mountain ranges, especially those of Tertiary date, is very much greater in magnitude and is different in nature and distribution from that which

would be given by the surficial cooling of the globe. The following quotations cover the principal points in the argument:

"The argument for the contractional hypothesis presupposes that the earth-mass may be considered as consisting of two portions, a cooled exterior of undetermined (though probably comparatively small) depth, inclosing a hot nucleus. . . . The secular loss of heat, it is assumed, would be greater from the hot nucleus than from the exterior, and the greater consequent contraction of the nucleus would therefore gradually withdraw the support of the exterior, which would collapse. The resulting strains upon the exterior would be mainly tangential. Owing to considerable inequalities in the ability of different portions to resist the strains thus developed, the yielding would take place at the lines, or regions of least resistance, and the effects of the yielding would be manifested chiefly, or wholly, at those places, in the form of mountain chains, or belts of table-lands, and in the disturbances of stratification. The primary division of the surface into areas of land and water are attributed to the assumed smaller conductivity of materials underlying the land, which have been left behind in the general convergence of the surface toward the center. Regarding these as the main and underlying premises of the contractional argument, it is considered unnecessary to state the various subsidiary propositions which have been advanced to explain the determination of this action to particular phenomena, since the main proposition upon which they are based is considered untenable.

There can be no reasonable doubt that the earth-mass consists of a cooled exterior inclosing a hot nucleus, and a necessary corollary to this must be secular cooling, probably accompanied by contraction of the cooling portions. But when we apply the known laws of thermal physics to ascertain the rate of this cooling, and its distribution through the mass, the objectionable character of the contractional hypothesis becomes obvious.

That Fourier's theorem, under the general conditions given, expresses the normal law of cooling, is admitted by all mathematicians who have examined it. The only ground of controversy must be upon the values to be assigned to the constants. But there seem to be no values consistent with probability which can be of help to the contractional hypothesis. The application of the theorem shows that below 200 or 300 miles the cooling has, up to the present time, been extremely little. . . . At present, however, the unavoidable deduction from this theorem is that the greatest possible contraction due to secular cooling is insufficient in amount to account for the phenomena attributed to it by the contractional hypothesis.

The determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated. It has been assumed that if a contraction of the interior were to occur, the yielding of the outer crust would take place at localities of least resistance. But this could be true only on the assumption that the crust could have a horizontal movement in which the nucleus does not necessarily share. A vertical section through the Appalachian region and westward to the 100th meridian shows a surface highly disturbed for about two hundred and fifty miles, and comparatively undisturbed for more than a thousand. No one would seriously argue that the contraction of the nucleus had been confined to portions underlying the disturbed regions: yet if the contraction was general, there must have been a large amount of slip of some portion of the undisturbed segment over the nucleus. Such a proposition would be very difficult to defend, even if the premises were granted. It seems as if the friction and adhesion of the crust upon the nucleus had been overlooked. Nor could this be small, even though the crust rested upon liquid lava. The attempts which some eminent geologists have recently made to explain surface corrugation by this method clearly show a neglect on their part to analyze carefully the system of forces which a contraction of the nucleus would generate in the crust. Their discussions have been argumentative and not analytical. The latter method of examination would have shown them certain difficulties irreconcilable with their knowledge of facts. Adopting the argumentative mode, and in conformity with their view regarding the exterior as a shell of insufficient coherence to sustain itself when its support is sensibly diminished, the tendency of corrugation to occur mainly along certain belts, with series of parallel folds, is not explained by assuming that these localities are regions of weakness. For a shrinkage of the nucleus would throw each elementary portion of the crust into a state of strain by the action of forces in all directions within its own tangent plane. A relief by a horizontal yielding in one direction would by no means be a general relief."

Dutton's criticisms robbed the current hypothesis of mountain-making of its conventional basis without providing a new foundation. It was a quarter of a century in advance of its time, has been seldom cited, and seems to have had but little direct influence in shaping subsequent thought. It, however, gave direction to Dutton's views, and his later papers were far-reaching in their influence.

If contraction from external cooling is not the cause

of the compressive forces it is necessary to seek another cause. Two years later, in 1876, Dutton attempted to provide an answer to this open question.[14] A review of this paper, evidently by J. D. Dana, is given in the Journal. The following explanations of Dutton's theory and of Dana's comments upon it are contained in a few paragraphs from this review (**12**, 142, 1876).

"Captain Dutton presents in this paper the views brought out in his article in volume viii of this Journal, with fuller illustrations, and adds explanations of his theory of the origin of mountains. The discussion should be read by all desiring to reach right conclusions, it presenting many arguments from physical considerations against the contraction-theory, or that of the uplifting and folding of strata through lateral pressure. There is much to be learned before any theory of mountain-making shall have a sufficient foundation in observed facts to demand full confidence, and Captain Dutton merits the thanks of geologists for the aid he has given them toward reaching right conclusions. His discussions are not free from misunderstandings of geological facts, and if they fail to be finally received it will be for this reason.

We here give in a brief form, and nearly in his own words, the principal points in his theory of mountain-making as explained in the later part of his memoir.

Accepting the proposition that there is a plastic condition of rock beneath the earth's crust and that metamorphism is a 'hydrothermal process,' and believing that 'the penetration of water to profound depths [in the earth's crust] is a well sustained theory,' he says that great pressure and a temperature approaching redness are essential conditions of metamorphism. . . . 'The heaviest portion would sink into the lighter colloid mass underneath, protruding it laterally beneath the lighter portions where, by its lighter density, it tends to accumulate.' He adds: 'The resulting movements would be determined, first, by the amount of difference in the densities of the upper and lower masses, and, second, by inequalities in the thickness of the strata: the forces now become adequate to the building of mountains and the plication of strata, and their modes of operation agree with the classes of facts already set forth as the concomitants of those features.'

The views are next applied to a system of plications. 'It has been indicated that plications occur where strata have rapidly accumulated in great volume and in elongated narrow belts; that the axes of plications are parallel to the axes of maximum deposit; and that the movements immediately followed the

deposition'—the case of the Appalachians being an example in which the accumulations averaged 40,000 feet. He observes: 'Wherever the load of sediments becomes heaviest, there they sink deepest, protruding the colloid magma beneath them to the adjoining areas, which are less heavily weighted, forming at once both synclinals and anticlinals.'

With regard to this new theory, we might reasonably question the existence of the colloid magma—a condition fundamental to the theory—and his evidence that water penetrates to profound depths in the earth's crust sufficient to make hydrous rocks. We might ask for evidence that the rocks beneath the Cretaceous and Tertiary. and other underlying strata of the Uintahs, were in such a colloid state, and this so near the surface, that the 'beds subsided by their gross weight as rapidly as they grew.'

Again, he says that the movements of mountain-making 'immediately followed the deposition.' 'Immediately' sounds quick to one who appreciates the slowness of geological changes. The Carboniferous age was very long; and somewhere in that part of geological time, either before the age had fully ended, or some time after its close, the epoch of catastrophe began."

We see foreshadowed in this paper the theory of isostasy, or condition of vertical equilibrium in the crust which Dutton published in 1889. This theory has borne remarkable fruit, but Dutton attempted to link to it the horizontally compressive forces which have produced folding and overthrusting. Willis in 1907[15] and Hayford in 1911, overlooking Dana's objections, have attempted to make a lateral isostatic undertow the cause of all horizontal movements in the crust, adopting the mechanism of Dutton. The present writer, although accepting the principle of isostasy as an explanation of broad vertical movements, has published papers which go to show the inadequacy of this hypothesis of lateral pressure; inadequate in time relation, in amount, and in expression.[16]

In 1903 it was determined by several physicists that the materials of the earth's crust were radioactive and must generate throughout geologic time a quantity of heat which perhaps equalled that lost by radiation into space. By 1907 this had become demonstrated. The remarkable conclusion had been reached that the earth, although losing heat, is not a cooling globe. Dutton's contentions against mountain growth through external cooling and contraction were thus unexpectedly,

through a wholly new branch of knowledge, demonstrated to be true.

Nevertheless, all students of orogeny are agreed that profound compressive forces have been the chief agents in developing mountain structures. Chamberlin was the first to arrive at the idea that the shrinkage may originate in the deeper portions of the earth under the urgency of the enormous pressures, apparently by giving rise to slow recombinations of matter into denser forms.[17]

The New Era in the Interpretation of Mountain Structures.

In the meantime, between 1874 and 1904, another advance in the knowledge of mountain structures was taking place in Europe. Suess studied the distribution of mountain arcs over the earth and dwelt upon the prevalence of overthrust structures; the backland being thrust toward and over the foreland, the rise of the mountain arc or geanticline depressing the foredeep or geosyncline. Bertrand and Lugeon from 1884 to 1900 were reinterpreting the Alpine structures on this basis. They showed that the whole mountain system had been overturned and overthrust from the south to an almost incredible degree. Enormous denudation had later dissevered the northern outlying portions and given rise to "mountains without roots,"—isolated outliers, consisting of overturned masses of strata which had accumulated as sediments far to the southward in another portion of the ancient geosyncline.

On a smaller scale similar phenomena are exhibited in the Appalachians. Willis showed that the deep subsidence of the center of the geosyncline gave an initial dip which determined the position of yielding under compression. Laboratory experiments brought out the weakness of the stratigraphic structure to resist horizontal compression. The nature of the stratigraphic series was shown to determine whether the yielding would be by mashing, competent folding, or breakage and overthrust. The problem of mountain structures was thus brought into the realm of mechanics. These results were published in three sources in 1893,—the Transactions of the

American Institute of Mining Engineers, the thirteenth annual report of the United States Geological Survey, and the Journal (**46**, 257, 1893).

Finally should be noted the contributions of the Lake Superior school of geology, in which the work of Van Hise stands preëminent. Under the economic stimulus given by the discovery and development of enormously rich bodies of iron ore, hidden under Pleistocene drift and involved in the complex structures of vanished mountain systems of ancient date, structural geology and metamorphism have become exact sciences to be drawn upon in the search for mineral wealth and yielding also rich returns in a fuller knowledge of early periods of earth history.

Crust Movements as Revealed by Physiography.

During the last quarter of the nineteenth century another division of geology, dominantly American, was taking form and growth,—the science of land forms,—physiography. The history of that development is treated by Gregory in the preceding chapter but some of its bearings upon theory, in so far as they affect the subject of mountain origin, are necessarily given here.

Powell, Dutton, and Gilbert in their explorations of the West saw the stupendous work of denudation which had been carried to completion again and again during the progress of geologic time. The mountain relief consequently may be much younger than the folding of the rocks, and may be largely or even wholly due to recurrent plateau movement, a doctrine to which Dana had previously arrived. But the introduction of the idea of the peneplain opened up a new field for exploration in the nature and date of crust movements. Davis by this means began to study the later chapters of Appalachian history, the most important early paper being published in 1891.[18] Since then Davis, Willis, and many others have found that, girdling the world, a large part of the mountainous relief is due to vertical elevatory forces acting over regions of previous folding and overthrust. In addition, great plateau areas of unfolded rocks have been bodily lifted one to two miles, or more, above their earlier levels.

They may be broad geanticlinal arches or bounded by the walls of profound fractures.

The linear mountain systems made from deep troughs of sediments have come then to be recognized as but one of several classes of mountains. This class, from its clear development in the Appalachians, and the fact that many of the laws of mountain structure pertaining to it were first worked out there, has been called by Powell the Appalachian type (**12**, 414, 1876). A classification of mountain systems was proposed by him in which mountains are classified into two major divisions, those composed of sedimentary strata altered or unaltered, and those composed in whole or in part of extravasated material. The first class he subdivides into six sub-classes of which the folded Appalachians illustrate one. It appears to the writer that Powell's classification gives disproportionate importance to certain types which he described; but nevertheless, the fact that such a classification was made, indicates the growth of a more comprehensive knowledge of mountains,—their origin, structure, and history.

Relations of Crust Movements to Density and Equilibrium.

A recent important development in the fields of geophysics and major crust movements consists in the incorporation into geology of the doctrine of isostasy. The evidence was developed in the middle of the nineteenth century by the geodetic survey of India which indicated that the Himalayas did not exert the gravitative influence that their volume called for. It was clear that the crust beneath that mountain system was less dense than beneath the plains of India and still less dense than the crust beneath the Indian Ocean. This relation between density and elevation indicated some approach to flotational equilibrium in the crust, comparable in its nature though not in delicacy of adjustment to the elevation of the surface of an iceberg above the ocean level owing to its depth and its density, less than that of the surrounding medium. This important geological conception was kept within the confines of astronomy and geodesy, however, until Dutton in 1876, but especially in 1889, brought

it into the geologic field. A test of isostasy was made for the United States by Putnam and Gilbert in 1895 and much more elaborate investigations have since been made by Hayford and Bowie. These investigations demonstrate the importance and reality of broad warping forces acting vertically and related to the regional variations of density in the crust.

There are consequently two major and unrelated classes of forces involved in the making of mountain structures, — the irresistible horizontal compressive forces, arising apparently from condensation deep within the earth, and vertical forces originating in the outer envelopes and tending toward a hydrostatic equilibrium. In this latter field of investigation, America, since the initial paper by Dutton, has taken the lead.

Conclusion on Contributions of America to Theories of Orogeny.

The sciences arose in Europe, but those which treated of the earth were still in their infancy when transplanted to America. The first comprehensive ideas on the nature of mountain structures arose in Great Britain and France. These ideas served as a guide and stimulus to observation in the recognition of deformations in the strata of the Appalachian system. Since 1840, however, America has ceased to be a pupil in this field of research but has joined as an equal with the two older countries. New ideas have been contributed, new and striking illustrations cited, first by the scientists of one nation, next by those of another. The composite mass of knowledge has grown as a common possession. Nevertheless, a review of the progress since 1840 as measured by the contribution of new ideas shows on the whole America at least equal to its intellectual rivals, and at certain times actually the leader. This is true of the science of geology as a whole and also of the subdivision of orogeny.

Thus far no mention has been made of German geologists, with the exception of Suess, an Austrian. German geology is voluminous and the names of many well-known geologists could be cited. But this article has sought to trace the origin and growth of fundamental ideas.

The Germans have been assiduous observers of detail; preëminent as systematizers and classifiers, seldom originators. Even petrology, which might be regarded as their especial field, was transplanted from Great Britain. In the science of mountains they have followed in their fundamental ideas especially the French.

Turning to the mediums of publication through which this progress of knowledge in earth structures has been recorded, the American Journal of Science stands foremost as the only continuous record for the whole century in American literature, fulfilling for this country what the Quarterly Journal of the Geological Society has done for Great Britain since 1845, and the Bulletin de la Société Géologique for France since 1830.

Notes.

[1] H. D. Rogers, Geology of New Jersey, Final Report, p. 115, 1840.

[2] H. D. Rogers, Geology of Pennsylvania, vol. 2, pt. II, pp. 761, 762, 1858.

[3] Connecticut Academy of Arts and Sciences, 1810; quoted by G. P. Merrill in Contributions to the History of North American geology, Ann. Rpt. Smithsonian Institution for 1904, p. 216.

[4] A Sketch of the geology, mineralogy, and scenery of the regions contiguous to the river Connecticut; with a geological map and drawings of organic remains; and occasional botanical notices, the Journal, 6, 1-86, 201-236, 1823; 7, 1-30, 1824.

[5] Clarence King, U. S. Geol. Exploration of the Fortieth Parallel, vol. 1, pp. 16, 44-48, 1878.

[6] Illustrations of the Huttonian Theory of the Earth, pp. 219 238, 1802.

[7] Robert Jameson, Elements of Geognosy, pp. 55-57, 1808.

[8] G. P. Merrill, Contributions to the History of American Geology. Report of the U. S. National Museum for 1904, p. 328.

[9] H. D. Rogers, Geology of Pennsylvania, vol. 2, p. 916, 1858.

[10] James Hall, Natural History of New York, Paleontology, vol. 3, pp. 51-73, 1859.

[11] The Journal, 5, 423-443, 474, 475; 6, 6-14, 104-115, 161-172, 304, 381, 382, 1873.

[12] C. R. Van Hise, Principles of North American Pre-Cambrian Geology, U. S. Geol. Surv., 16th Ann. Report, pt. I, pp. 607-612, 1896.

[13] W. N. Rice, On the use of the words synclinorium and anticlinorium, Science, 23, 286, 287, 1906.

[14] C. E. Dutton, Critical observations on theories of the earth's physical evolution, The Penn Monthly, May and June, 1876.

[15] B. Willis, Research in China, vol. 2, 1907.

[16] Joseph Barrell, Science, 39, 259, 260, 1909; Jour. Geol., 22, 672-683, 1914.

[17] T. C. Chamberlin, Geology, vol. 1, pp. 541, 542, 1904.

[18] W. M. Davis, The geological dates of origin of certain topographic forms on the Atlantic slope of the United States, Geol. Soc. Am. Bull., 2, 541-542, 545-586, 1891.

V

A CENTURY OF GOVERNMENT GEOLOGICAL SURVEYS

By GEORGE OTIS SMITH

Director of the United States Geological Survey

EVEN a Federal Bureau must be considered a product of evolution: the past of the United States Geological Survey far antedates March 3, 1879. The scope of endeavor, the refinement of method, and especially the personnel of the newly created service of that day were largely inherited from pioneer organizations. Therefore a review of the country's record of surveys under Government auspices becomes more than a grateful acknowledgment by the present generation of geologists of the credit due to those who blazed the way; it shows the sequence and progress in the contributions made by geologic science to industry.

The earlier stages in industrial evolution mentioned by Hess[1]—exploitation, development, and maturity—determine a somewhat similar progressive development in geologic investigation, so that geographic exploration and geologic reconnaissance of the broadest type are the normal contribution of exact science whenever and wherever a nation is in the state of exploitation and initial development of its mineral and agricultural resources. The refinements of detailed surveys and quantitative examinations belong rather to the next stage of intensive utilization, or, indeed, they are the essentials preliminary to full use. Thus regrets that the results of present-day work were not available fifty years ago are largely vain: the fathers may not have been without the vision; they simply did the work as their day and generation needed it done.

Twenty years ago S. F. Emmons, in a presidential address before the Geological Society of Washington, divided the history of Governmental surveys in this country into two periods, separated in a general way by the Civil War. The first of these was the period of geographic exploration, the second that of geologic exploration. Mr. Emmons of course regarded this subdivision as not hard and fast, yet his dividing line seems logical, for not only did the military activities in the East necessarily suspend exploration in the West, but after the war national, political, and economic considerations led naturally to the demand for a more exact knowledge of the vast national domain in the West. Geography and geology are so closely related that Mr. Emmons's distinction of the two periods is useful only with the limitations inferentially set by himself—namely, that while geologic investigation entered into most of the explorations of the earlier period, the geologist was regarded as only an accessory in these exploring expeditions; on the other hand, in the later surveys the topographic work was developed because it was essential to the geologic investigations.

The year 1818 was a notable one in American geology, first of all in the appearance of the American Journal of Science, itself so perfect a vehicle for geological thought that, as is so well stated by Dr. G. P. Merrill, "a perusal of the numbers from the date of issue down to the present time will alone afford a fair idea of the gradual progress of American geology." The beginning of publications on New England geology appeared that year in Edward Hitchcock's first paper on the Connecticut Valley (**1**, 105, 1818) and the Danas' (S. L. and J. F.) detailed geologic and mineralogic description of Boston and vicinity; and the "Index" of Amos Eaton (noticed in this Journal, **1**, 69) was the first of that long list of notable contributions to American stratigraphy that are to be credited to the New York geologists.

In the present discussion, too, the year 1918 can be regarded as in a way the centennial of Government geologic surveys, for it was in 1818 that Henry R. Schoolcraft began his trip to the Mississippi Valley—perhaps the first geologic reconnaissance into the West—and it

was his work in the lead region which served to make him a member of the Cass expedition sent out by the Secretary of War in 1820 to examine the metallic wealth of the Lake Superior region. The earlier Government explorations of Lewis and Clark, in 1803-7, and of Pike, in 1805-7, were so exclusively geographic that geologic work under Federal auspices must be regarded as beginning with Schoolcraft and with Edwin James, the geologist of the expedition of Major Long in 1819-20 to the Rocky Mountains. Both these observers published reports that are valuable as contributions to the knowledge of little-known regions.

Any description of geologic work under the Federal Government that included no reference to the State surveys would be inadequate, for in both date of execution and stage of development the work of the State geologists must be given precedence. In Merrill's Contributions to the History of American Geology,[2] whose modest title fails even to suggest that this work not only furnishes the most useful chronologic record of the progress of the science on the American continent but is in fact a very thesaurus of incidents touching the personal side of geology, the author by his division of his subject shows that four decades of the era of State surveys elapsed before the era of national surveys began.

Thus the geologic surveys of some of the Eastern States antedate by several decades any Federal organization of comparable geologic scope, and in investigations directed to local utilitarian problems these pioneer geologists working in the older settled States of the East were in fact already conducting work as detailed in type as much of that attempted by the Federal geologists of the later period. Even to-day it is true in a general way that the State geologist can and should attack many of his local problems with intensive methods and with detail of results that are neither practicable nor desirable for the larger interstate investigations or for examinations in newer territory. All this relation of State and Federal work must be looked upon as normal evolutionary development of geologic science in America.

One who reads the names of the Federal geologists of the early days, beginning with Jackson and Owen and

following with such leaders in Federal work as Gilbert, Chamberlin, King, R. D. Irving, Pumpelly, Van Hise, and Walcott, may note that these were all connected in their earlier work with State surveys. Nor has the relation been one-sided, for among the State geologists Whitney, Blake, Mather, Newberry, J. G. Norwood, Purdue, Bain, Gregory, Ashley, Kirk, W. H. Emmons, DeWolf, Mathews, Brown, Landes, Moore, and Crider received their field training in part or wholly as members of a Federal Survey. Moreover, under the present plan of effective coöperation of several of the State surveys with the United States Geological Survey, it is often difficult to differentiate between the two in either personnel or results, for it even happens that the publishing organization may not have been the major contributor. The full record of American geology, past and present, can not be set forth in terms of Federal auspices alone.

The three decades preceding the Civil War, then, constitute the era of State surveys, well described by Merrill as at first characterized by a contagious enthusiasm for beginning geologic work, later by a more normal condition in which every available geologist seems to have been quietly at work, and finally by renewed activity in creating new organizations. The net result was that Louisiana and Oregon seem to have been the only States not having at least one geological survey.

The first specific appropriation by the Federal Government for geologic investigation appears to have been made in 1834, when a supplemental appropriation for surveys of roads and canals under the War Department, authorized in 1824, contained the item "of which sum five thousand dollars shall be appropriated and applied to geological and mineralogical survey and researches." In July, 1834, Mr. G. W. Featherstonhaugh was appointed United States geologist and employed under Colonel Abert, U. S. Topographical Engineers, to "personally inspect the mineral and geological character" of the public lands of the Ozark Mountain region. Overlooking the incidental fact that this Englishman—a man of scientific attainment and large interest in public affairs—was never naturalized,[8] it must be placed to the credit of this first of United States geologists that within seven

J. V. Hayden

From "Contributions to the History of American Geology"
by George P. Merrills.

months he completed his field work and returned to Washington, and on February 17, 1835, his report was transmitted to Congress. Two years earlier Featherstonhaugh had memorialized Congress for aid in the preparation of a geologic map of the whole territory of the United States, and in connection with this project he suggested that geology as an aid to military engineering should have a place in the curriculum at West Point. This first United States geologist also appears to have combined an appreciation of the practical worth of "the mineral riches of our country, their quality, quantity, and the facility of procuring them," with an interest in the more scientific side of geology, though his hypotheses regarding both economic geology and stratigraphic and structural geology have not won the endorsement of all later workers in the same regions. In all these respects, however, Featherstonhaugh may stand as a fairly good prototype. His contributions to international affairs subsequent to his scientific service to the United States are of interest; he served as one of Her Majesty's commissioners in the settlement of the Canadian-United States boundary question in 1839-40 and made an examination of the disputed area, and after the settlement of this controversy he was appointed British Consul for the Department of the Seine, France, where in 1848 he personally engineered the escape of Louis Philippe from Havre.

The Federal geologic work thus started was soon continued in surveys of wider scope and more thorough accomplishment. The position of the Government as the proprietor of mineral lands in the Upper Mississippi Valley led to their examination. These Government lands containing lead had been reserved from sale for lease since 1807, although no leases were issued until 1822. The amount of illegal entry and consequent refusal of smelters and miners to pay royalty after 1834 forced the issue upon the attention of Congress, and in 1839 President Van Buren was requested to present to Congress a plan for the sale of the public mineral lands. In carrying out this policy Dr. David Dale Owen was selected to make the necessary survey.

Owen had served as an assistant on the State Survey

of Tennessee and as the first State geologist of Indiana, and he organized the new work promptly and effectively. Although suffering from the handicap unfortunately known by geologists of the present day—the receipt late in the season (August 17, 1839) of authority to begin work—within exactly a month he had his force of 139 assistants organized into 24 field parties, instructed in "such elementary principles of geology as were necessary to their performance of the duties required of them." His plan of campaign provided for a northward drive at a predetermined rate of traverse for each party, with periodic reports to himself at appointed stations, "to receive which reports and to examine the country in person" he crossed the area under survey eleven times. The result of such masterful leadership was the completion of the exploration of all the lands comprehended in his orders in two months and six days, and his report on this great area—about 11,000 square miles—bears date of April 2, 1840.

Eight years later Doctor Owen made a survey of an even larger area, continuing his examination northward to Lake Superior. Again his report was published promptly, and he continued for several years his examination of the Northwest Territory, submitting his final report in 1851. It is interesting to note that in his earlier report Doctor Owen subscribed himself as "Principal Agent to explore the Mineral Lands of the United States," but that in the later report he was "U. S. Geologist for Wisconsin." The two surveys together covered 57,000 square miles.

During the same period similar surveys were being made in northern Michigan by Dr. Charles T. Jackson, 1847-48, and Foster and Whitney, 1849-51. These surveys also had been hastened by the "copper fever" of 1844-46, with wholesale issue of permits and leases, Congress in 1847 authorizing the sale of the mineral lands and a geological survey of the Lake Superior district. The execution of these surveys under Jackson and under Foster and Whitney and the prompt publication in 1851 of the maps of the whole region materially helped to establish copper mining on a more conservative basis.

and the development of the Lake Superior region was rapid.[4]

These land-classification surveys, with their definite purpose, represent the best geologic work of the time. The plan necessitated thoroughgoing field work with considerable detail and prompt publication of systematic reports, and in the working up of the results specialists like James Hall and Joseph Leidy contributed, while F. B. Meek was an assistant of Owen. It is worthy of note that had not Doctor Houghton, the State geologist of Michigan, met an untimely death in 1847, effective coöperation of the State Survey with the Federal officials would have combined geologic investigation with the execution of the linear surveys.[5]

Belonging to the same period of geologic exploration was the service of J. D. Dana, as United States Geologist on the Wilkes Exploring Expedition, the disaster to which compelled his return from the Pacific Coast overland and resulted in his geologic observations on Oregon and northern California.

The military expeditions during the decade 1850-60 and the earlier expeditions of Frémont added to the geographic knowledge of the Western country and also contributed to geologic science, largely through collections of rocks and fossils, usually reported on by the specialists of the day. Thus the names of Hall, Conrad, Hitchcock, and Meek appear in the published reports on these explorations, while Marcou, Blake, Newberry, Gibbs, Evans, Hayden, Parry, Shumard, Schiel, Antisell, and Engelmann were geologists attached to the field expeditions. In 1852 geologic investigation was seemingly so popular as to necessitate the statutory prohibition "there shall be no further geological survey by the Government unless hereafter authorized by law."

Certain of these explorations had a specific purpose: several of them sought a practical route for a transcontinental railroad; another a new wagon road across Utah and Nevada; and one under Colonel Pope, with G. G. Shumard as geologist, was sent out "for boring Artesian Wells along the line of the 32d Parallel" in New Mexico. The pub-

lished reports varied greatly in scientific value and in carefulness of preparation, while the publication of at least two reports was delayed until long after the war, and the manuscript of another was lost. The report of the expedition of Major Emory contained a colored geologic map of the western half of the country, a pioneer publication, for the map prepared by Marcou extended only to the 106th meridian.

Thus in the first period of Government surveys, covering about forty years, the great West, with its wealth of public lands, was well traversed by exploratory surveys, which furnished, however, only general outlines for a comprehension of the stratigraphy and structure of mountain and valley, plain and plateau. To an even less degree was there any realization of the economic possibilities of the vast territory west of the Mississippi. President Jefferson, in planning the Lewis and Clark expedition, had stated his special interest in the mineral resources of the region to be traversed. Nearly forty years later Doctor Owen was strongly impressed with the commercial promise of the region he surveyed. His reports contain analyses of ores and statistics of production; he compared the lead output of Wisconsin, Iowa, and Illinois with that of Europe and foretold the value of the iron, copper, and zinc deposits of the area; he outlined the extent of the Illinois coal field; and he laid equal emphasis upon the agricultural possibilities of the region. Indeed, so optimistic were Owen's general conclusions that he referred to his separate township plats, with their detailed descriptions, as the basis for his sanguine opinions, realizing that "the explorer is apt to become the special pleader." With equal breadth of view and thoroughness of execution the surveys of Foster and Whitney laid the foundation for the development of the copper and iron resources of the Lake Superior region, and although these areas were largely wilderness and not adapted to rapid traverse or easy observation the reports on their explorations nevertheless compare most favorably with the contributions of geologists working in the more hospitable regions in the older States.

The period following the Civil War naturally became one of national expansion, the faces of many were turned

westward, and exploration of the national domain for its industrial possibilities took on fresh interest. Home-seekers and miners largely made up this army of peaceful invasion, and the winning of the West began on a scale quite different from that of the days of the military path-finding expeditions of Frémont and other Army officers. Thus the nation was aroused to the task of investigating its public lands and Congress gave the support needed to make geologic exploration possible on a large scale.

Geologic surveys of a high order were continued in the older States, as shown by the contributions during this period of J. P. Lesley and G. H. Cook in the East, W. C. Kerr, E. W. Hilgard, and E. A. Smith in the South, and J. S. Newberry, C. A. White, Raphael Pumpelly, T. C. Chamberlin, Alexander Winchell, and T. B. Brooks in the Central States. To the north the Canadian Survey, organized in 1841 under Logan, had continued under the same sturdy leadership until 1869, when the experienced and talented Doctor Selwyn became Director. As contrasted with the short careers of most of the State Surveys and with the temporary character of all of the Federal undertakings in geologic investigation, the continuance of the Canadian Geological Survey for more than half a century under two directors gave opportunity for continuity of effort in making known to the people of the Dominion its resources and at the same time contributing to the world much pure science.

Passing with simple mention the two Government expeditions into the Black Hills, which afforded opportunity for geologic exploration by N. H. Winchell in 1874 and by Jenney and Newton in 1875, the record of geologic work under Government auspices in the period immediately following the Civil War groups itself around the names of four leaders—Hayden, King, Powell, and Wheeler. The four organizations, distinguished commonly by the names of these four masterful organizers, occupied the Western field more or less continuously from 1867 to 1878, and the sum total of their contributions to geography and geology was large indeed. In the words of Clarence King,[6] "Eighteen hundred and sixty-seven, therefore,

marks, in the history of national geological work, a turning point, when the science ceased to be dragged in the dust of rapid exploration and took a commanding position in the professional work of the country.'' Together these four expeditions covered half a million square miles, or more than a third of the area of the United States west of the one-hundredth meridian, and the cost of all this work was approximately two million dollars, which was a small fraction of its value to the nation counting only the impetus given to settlement and utilization.

As viewed from a distance of nearly half a century, these four surveys differed much in plan of organization, scope of purpose, and success of execution, so that comparison would have little value except as possibly bearing upon the work of the larger organization which followed them and became the heir not only to much that had been attained by these pioneer surveys but also to the great task uncompleted by them. So, if in the earliest days of the present United States Geological Survey there may have been a certain partisanship in tracing derived characters in the new organization, it is even now worth while to recognize the real origin of much that is credited to present-day development.

Dr. F. V. Hayden was the first of these Survey leaders to engage in geological exploration. He visited the Badlands as early as 1853, and his connection with subsequent expeditions was interrupted only by his service as a surgeon in the Federal Army during the war. In 1867, however, Hayden resumed his geologic work as United States Geologist in Nebraska, operating under direction of the Commissioner of the General Land Office. In the following eleven years the activities of the Hayden Survey—the "Geological and Geographical Survey of the Territories"—extended into Wyoming, Colorado, New Mexico, Montana, and Idaho, covering with areal surveys 107,000 square miles. This Survey, as might be expected from the long experience of its leader, made large contributions to stratigraphy, which involved notable paleontologic work by Cope, Meek, and Lesquereux. Next in importance was the structural work of A. C. Peale, W. H. Holmes, Capt. C. E. Dutton, and Dr.

Hayden himself, and the influence of these expeditions in popularizing geology should not be overlooked. The expedition of 1871 into the geyser region on the upper Yellowstone resulted in the creation of the first of the national parks. W. H. Holmes began his artistic contributions to geology in 1872 with this Survey. Topographic mapping was added to the geologic exploration, James T. Gardner and A. D. Wilson joining the Hayden Survey after earlier service on the King Survey and Henry Gannett being a member of parties, first as astronomer and later as topographer in charge. The accomplishment of the Hayden Survey itself and the later work of many of its members show that this organization possessed a corps of strong men.

The King Survey was a smaller organization, with Congressional authorization of definite scope and a systematic plan of operation. The beginning of construction of the Union Pacific terminated the period of the railroad surveys under the War Department and afforded opportunity for geologic work that would be more than exploratory: the opening up of the new country made investigation of its resources logical. This fact was recognized by Clarence King, who had traversed the same route as a member of an emigrant train with his friend James T. Gardner. His plan to make a geological cross section of the Cordilleras, with a study of the resources along the route of the Pacific railroads, won the support of Congress, and the "Geological Exploration of the Fortieth Parallel" was authorized in 1867, with Clarence King as geologist in charge, under the Chief of Engineers of the Army. Field work was begun in the summer of that year, and it is interesting to note that Mr. King and his small force of geological assistants—the two Hagues and S. F. Emmons—began at the western end of this cross section, and in this and subsequent years extended the survey from the east front of the Sierra Nevada to Cheyenne, covering a belt of territory about 100 miles in width. This comprehensive plan was carried out in the field operations, and the scientific and economic results were systematically worked up in the reports, which appeared in 1870-80. The only departure from this plan was a study of the

volcanic mountains Shasta, Rainier, and Hood, in 1870, occasioned by an unexpected and unsolicited appropriation for field work, and that summer's work resulted in the discovery of active glaciers, the first known within the United States.

The Fortieth Parallel Survey is to be credited with contributions to the knowledge of the stratigraphy of the West, the region traversed being remarkably representative of the stratigraphic column, to which was added the paleontologic work of Marsh, Meek, Hall, and Whitfield, while the attempt was made to interpret the sedimentary record in terms of Paleozoic, Mesozoic, and Tertiary geography. King's plan of survey included large use of topographic mapping with astronomic base and triangulation control and contours based upon barometric elevations. The results were pronounced by an unfriendly critic[7] as "very valuable, especially from a geological point of view," but unfortunate in being the forerunner of work in which Government geologists "have presumed to arrogate the control of the fundamental operations of a topographic survey." To the King Survey must be credited the introduction of systematic contour mapping and the use of contour maps for purposes of geology. In two other respects the King Survey contributed largely to future Government work: microscopical petrography in the United States may be said to have begun with the visit of Professor Zirkel to this country as a member of this Survey in 1875, and the report of J. D. Hague on "Mining Industry" was the fitting expression of the emphasis then put on the study of the mineral resources of this newly opened territory, a subject of investigation that was in large part the true basis of King's project rather than simply "the immediate excuse for the Survey." An earlier influence in the scientific study of ore deposits had come from Von Richthofen's investigation of the Comstock Lode in 1865 and his subsequent work with Whitney in California. The incident of King's relation to the diamond fraud in Arizona in 1872 furnished a precedent for public servants of a later day; he investigated the reported find from scientific interest but exposed it with all the zeal of a publicist and truth lover. In a word, the Fortieth Parallel Sur-

J. W. Powell

vey commands our admiration for its brilliant plan, thoroughgoing work in field and office, and high quality of personnel.

Major J. W. Powell began his large contribution to Government surveys with his exploration of the Grand Canyon in 1869, the Congressional recognition of his expedition being limited to an authorization for the issue of rations by the War Department. Small appropriations were made in the following years, and in 1874 full authorization was given for the continuance of his survey in Utah under the Secretary of the Interior and was followed by the adoption of the name "United States Geographical and Geological Survey of the Rocky Mountain Region." This organization was the least pretentious of the four operating during this period—it covered less area, expended less public money, and published much less—but its contribution to American geology is not to be measured by miles or pages but by ideas. Its physical environment favored this survey, and in the work of Powell, Dutton, and Gilbert can be seen the beginnings of physiography on the heroic scale exemplified in the Grand Canyon and the High Plateaus. The first use of terms like "base level of erosion," "consequent and antecedent drainage," and "laccolith" marked the introduction of new ideas in the interpretation of land sculpture and geologic structure. The daring boat trip of Powell was no less brilliant than his simple explanation of the Grand Canyon itself.

"The United States Geographical Surveys West of the 100th Meridian" was the title given to the explorations made under Lieut. G. M. Wheeler, of the Engineer Corps, which began with topographic reconnaissances in Nevada, Utah, and Arizona, specifically authorized by Congress in 1872. From the standpoint of American geology this could be better known as the Gilbert Survey, Mr. G. K. Gilbert serving for the three years 1871-73, the later part of the time with the title of chief geological assistant. Gilbert's contributions included his description of Basin Range structure, his first account of old Lake Bonneville, and his discussion of the erosion phenomena of the desert country. J. J. Stevenson also served later as a geologist of this Survey, and A. R. Mar-

vine, E. E. Howell, E. D. Cope, Jules Marcou, and I. C. Russell were connected with the field parties. Captain Wheeler's own claim for the work of his Survey emphasized its geographic side, for he regarded the results as the partial completion of a systematic topographic survey of the country.

By 1878, when the Fortieth Parallel Survey had completed the work planned by its chief, three of these independent surveys still contended for Federal support and for scientific occupation of the most attractive portions of the Western country. Unrestrained competition of this kind, even in the public service, proves as wasteful as unregulated competition in private business,[8] and Congress appealed to the National Academy of Sciences for a plan for Government surveys to "secure the best results at the least possible cost." Under instructions by Congress the National Academy considered all the work relating to scientific surveys and reported to Congress a plan prepared by a special committee, whose membership included the illustrious names of Marsh, Dana, Rogers, Newberry, Trowbridge, Newcomb, and Agassiz. This report, which was adopted by the Academy with only one dissenting vote, grouped all surveys—geodetic, topographic, land parceling, and economic—under two distinct heads, surveys of mensuration and surveys of geology. At that time five independent organizations in three different departments were carrying on surveys of mensuration, and the Academy recommended that all such work be combined under the Coast and Geodetic Survey with the new name Coast and Interior Survey. For the investigation of the natural resources of the public domain and the classification of the public lands a new organization was proposed, the United States Geological Survey. The functions of these two surveys and of a third coördinate bureau in the Interior Department, the Land Office, were carefully defined and their interrelations fully recognized and provided for in the plan presented to Congress. Viewed in the light of 39 years of experience the National Academy plan would be indorsed by most of us as eminently practical, and the report stands as a splendid example of public service rendered by America's leading scientists. The legislation

which embodied the entire plan, however, failed of passage in Congress.

The natural activity behind the scenes of the conflicting interests represented by those connected with the several surveys may be seen in the legislative history of the moves leading up to the creation of the United States Geological Survey. In the last session of the 45th Congress the special legislation embodying the recommendations of the National Academy was included in the Legislative, Executive, and Judicial Appropriation bill as it passed the House of Representatives, while the Sundry Civil Appropriation bill carried an item simply making effective the longer section in the other appropriation bill. The item in the Legislative appropriation bill created the office of the Director of the Geological Survey, provided his salary, and defined his duties, as well as specifically terminating the operations of the three older organizations. The item in the Sundry Civil bill as it passed the House appropriated $100,000 for the new Geological Survey, but when this appropriation bill was reported to the Senate a committee amendment added the words "of the Territories," and further amendments offered on the floor changed the item so as to provide specifically and exclusively for the continuation of the Hayden Survey. Other amendments provided small appropriations for the completion of the reports of the Powell and Wheeler surveys, and the bill passed the Senate in this form. The Legislative Appropriation bill was similarly pruned, while in the Senate, of all reference to the proposed new organization. This bill, however, died in conference, but in the last hours of the session the conferees on the Sundry Civil bill took unto themselves legislative powers and transferred from the dead bill to the pending measure all the language which constitutes the "organic act" of the United States Geological Survey. This action was denounced in the Senate as "a wide departure from the authority that is possessed by a conference committee," and it was further stated in debate that the inserted provision which created a new office and discontinued the existing surveys was one "which neither the Committee of the Senate nor the Senate itself ever saw." This assertion was perhaps par-

liamentarily sound in that the language was new to the Sundry Civil bill, yet actually the Senate had only two days before stricken the same proposed legislation from the pending Legislative Appropriation bill. However, the House conferees—Representatives Atkins of Tennessee, Hewett of New York, and Hale of Maine—had realized their tactical advantage, and the Senate, after a brief debate, voted on March 3 to concur in the report of the committee of conference, thus reversing all their earlier action, in which the friends of the Hayden and Wheeler organizations apparently had commanded more votes than the advocates of the National Academy plan.

Clarence King was appointed first Director of the United States Geological Survey on April 3, 1879, and began the work of organization. With his proven genius for administration, King promptly resolved the doubt as to the meaning of the term "national domain" in the language defining the duties of the Director by taking the conservative side and limiting the work of the new organization to the region west of the 102d meridian. This region was divided into four geological divisions, and for economy of time and money field headquarters were established for these divisions. The Division of the Rocky Mountains was placed under Mr. Emmons as geologist in charge, the Division of the Colorado under Captain Dutton, the Division of the Great Basin under Mr. Gilbert, and the Division of the Pacific under Arnold Hague. The Division of the Colorado was intended as merely temporary for the purpose of bringing to completion the scientific work of the Powell Survey. Similarly Dr. Hayden was given the opportunity to prepare a systematic digest of his scientific results. This organization of the work and the selection of geologists in charge showed the relation of the new and the old, and a glance at the personnel of the new Survey indicates the extent to which the geologic investigation of the Western country was to continue without interruption. Of the twenty-four geologists and topographers listed in the first administrative report, four had been connected with the Powell Survey, two with the Hayden, three with the Wheeler, and five with the King Survey.

In planning the initial work of the United States

Clarence King

Geological Survey, the Director speaks of the "most important geological subjects" and "mining industries," of "instructive geological structure" and "great bullion yield" in the same sentences, so that the intent was plain to make the geologic investigations both theoretical and practical.

It was expected that the field of operations of this Federal Survey would be at once extended by Congress over the whole United States, but the measure making this extension, which would simply carry out the intent of the framers of the legislation creating the new bureau. passed the House alone, and it was only by subsequent modification of the wording of appropriation items that the United States Geological Survey became national in scope as well as in name. The critical question of the effective coördination of State and Federal geologic surveys was met by Director King, who corrected an erroneous impression "industriously circulated" by stating his policy to be to urge the inauguration and continuance of State surveys.[9] This was the initial step in the coöperation between State and Federal surveys which became effective on a large scale in subsequent years.

Though the Geological Survey has extended its operations over the whole United States, its largest activities have always been directed toward the exploration and development of the newer territory in the public-land States. All four of its directors had their field training in the West: the name of Major Powell, who succeeded King in 1880, is inseparably connected with scientific exploration; Charles D. Walcott, who was Director from 1894 to 1907, the period of the Survey's greatest expansion, made the largest contribution to the Paleozoic stratigraphy and paleontology of the West; and the present Director spent seven field seasons in the Northern Cascades and one in a mining district in Utah. The scope of the activities both East and West as developed during the 39 years since the establishment of the new bureau can be best described, perhaps, in terms of its present functions as expressed in the organization of to-day.

The growth of the Survey is measured in the increase of annual appropriation from $106,000 in 1879-80 to the amount available for the current year—$1,925,520, not

including half a million dollars from War Department appropriations being spent in the topographic work of the Survey. The corresponding increase in personnel has been from 39, listed in the first report, to 911 holding regular appointments at the present time, divided among the different branches as follows: A scientific force of 173 in the Geologic Branch, 169 in the Water Resources Branch, 71 in the Topographic Branch, and 15 in the Land Classification Board, with a clerical force of 168 divided among the same branches, and the remainder the technical and clerical employees of the publication and administrative branches. These personnel statistics are not expressive of normal conditions, since a large number of the topographic engineers are commissioned officers and thus are not included on the civilian roll, while, on the other hand, the classification of the stock-raising homestead lands makes the technical force of the Water Resources Branch unusually large this year.

The primary aim of the Geological Survey is geologic, whether directed by authority of law toward the "examination of the geological structure, mineral resources, and products of the national domain," toward the preparation of the authorized "reports upon general and economic geology and paleontology," of the "geologic map of the United States," or of the "report on the mineral resources of the United States," or toward the "continuation of the investigation of the mineral resources of Alaska" or "chemical and physical researches relating to the geology of the United States." The spirit and the purpose of the Survey's work in all these fields are not believed to have materially changed from those of the founders of the science in America. From time to time too much emphasis may have appeared to be laid upon applied geology as contrasted with pure science, yet the report of the National Academy to Congress in terms placed the stress upon economic resources and referred to paleontology as "necessarily connected" with general and economic geology. The practical purpose of geologic research under Government auspices must be recognized by the administrator, whether he be the paleontologist like Wal-

cott, the philosopher like Powell, or the mining geologist like King. That the task of steering the true course is no new problem can be seen from the statement of Owen[10] written 70 years ago, and these words describe conditions of Government geological work even to-day:

> Scientific researches, which to some may seem purely speculative and curious, are essential as preliminaries to these practical results. Further than such necessity dictates, they have not been pushed, except as subordinate and incidental, and chiefly at such periods as, under the ordinary requirements of public service, might be regarded as leisure moments; so that the contributions to science thus incidentally afforded, and which a liberal policy forbade to neglect, may be considered, in a measure, a voluntary offering, tendered at little or no additional expense to the department.

The increased attention given to mineral resources has been a matter of gradual growth. Mr. King early organized a Division of Mining Geology with Messrs. Pumpelly, Emmons, and Becker as geologists in charge, to whom were assigned the collection of mineral statistics for the Tenth Census. These Survey geologists and Director King himself held appointments as special agents of the Census Bureau, and on the staff selected for this work appear the names of T. B. Brooks, Edward Orton, T. C. Chamberlin, Eugene A. Smith, George Little, J. R. Proctor, R. D. Irving, N. S. Shaler, John Hays Hammond, Bailey Willis, and G. H. Eldridge, indicating the extent to which the supervision of these inquiries was placed in the hands of economic geologists. This procedure was reverted to by Director Walcott and in the last ten years has become a well-established policy, the statistics of annual production of all the important mineral products being under the charge of geologists, as best qualified to comprehend the resources of the country. Another of these special assistants in 1880 was Albert Williams, Jr., who became the first chief of the Division of Mineral Resources, in 1882. The study of ore deposits, which may be said to have begun with the King Survey, was inspired by King's own appreciation of the broad geologic relations of the distribution of mineral wealth and by the detailed studies of individual

mining districts by his associates, "based upon facts accurately determined in the light of modern geology."

Geological surveys have been prosecuted in Alaska since 1895, and in the last few years the annual appropriation for the work has been the same as that made for the expenses of the whole Survey in the first year of its history. The Division of Alaskan Mineral Resources is in fact a geological survey in itself, except that it shares in the administrative machinery of the larger organization and has the advantage of the coöperation of the scientific specialists of the Survey as they may be needed to supplement its own force. All the investigations in this distant part of the country represent the Geological Survey at its best, for here the organization's long experience in the Western States can be applied to most effective and helpful work on the frontier, where the geologist and topographer in their exploration do not always follow the prospector but often precede him. Undoubtedly no greater factor has contributed to the development of Alaskan resources than this pioneer work of the Federal Survey, yet the work has also contributed notable additions to the sciences of geology and geography.

The first duty laid upon the Director of the Geological Survey in the law of 1879 was "the classification of the public lands," and this phrase undoubtedly expressed the idea of the committee of the National Academy. The same legislation, however, contained provision for the further consideration by a commission of the classification and valuation of the public lands, as also recommended by the National Academy. Thus the decision of Director King that the classification intended by Congress was scientific and was intended for general information and not to aid the Land Office in the disposition of land by sale or otherwise was really based upon the deliberate opinion of the Public Lands Commission, of which he was a member, that classification would seriously impede rapid settlement of the unoccupied lands. Nearly forty years later those who are intrusted with the land-classification work of the Geological Survey recognize this familiar argument, which undoubtedly had much more force in that earlier stage of the utilization of the

Nation's resources of land.[11] The conception of land classification as a business policy on the part of the Government as a landed proprietor belongs rather to this day of more intensive development. At present current public-land legislation calls for highest use, and hence official investigation of natural values and possibilities must precede disposition. This type of mineral and hydrographic classification of public lands has been in progress in increasing amount since 1906, so that now the Geological Survey is the kind of scientific adviser to the Secretary of the Interior and Commissioner of the General Land Office that may have been contemplated by the National Academy of Sciences in 1878. It is plain, however, to everyone at all conversant with Western conditions that the recent land-classification surveys in Wyoming, for instance—detailed geologic surveys which form the basis for the valuation of public coal lands in 40-acre units—would have possessed no utility in 1871, when the coal-land law was passed but when the demand for railroad fuel had just begun.

The land-classification idea is of course the basis of the National forest and irrigation movements. The laws of 1888 and 1896, which mark the beginning of active endorsement by Congress of these conservation movements, placed upon the Survey the duties of examining reservoir sites and forest reserves respectively. The earlier of these laws began the investigation of the water resources of the country, which is still an important phase of the Survey's activity, and led to the creation of an independent organization—the Reclamation Service. It is easy to trace the beginnings of Federal reclamation of arid lands in the pioneer work of Powell, whose report in 1878 on the arid region of the United States was the first adequate statement of the problem of largest use of these lands in terms broader than those of individualistic endeavor. For years, however, Powell's appeal for Congressional consideration of this National task was like the "voice of one crying in the wilderness."

In a somewhat similar way the forestry surveys under the Geological Survey helped in the organization of a separate bureau—now the Forest Service. The other

important Federal bureau tracing direct relationship to the Survey is the Bureau of Mines, established in 1910, which continued the investigations in mining technology specifically provided for by Congress for six years under the Geological Survey but in some degree begun in the early days of the Survey under Directors King and Powell.

Another equally important organization of a public nature, though not a Federal bureau, traces its beginnings to the Geological Survey: the Geophysical Laboratory of the Carnegie Institution, which now exercises so potent an influence over geologic investigation, had its origin in the official work of the Geological Survey's Division of Chemical and Physical Research, and its personnel was at first largely recruited from the Survey. The highly original experimental work of this laboratory has extended far beyond the scope of the Survey's work—at least far beyond the scope possible with the Federal funds available—yet most of the results of these investigations may eventually come under even a strict construction of the language used in the Survey's appropriation "for chemical and physical researches relating to the geology of the United States."

The topographic work of the present Survey continues with constant refinement of standards and economy of methods the work of the earlier organizations. The primary purpose of these topographic surveys is to provide the bases for geologic maps, yet these topographic maps, which cover 40 per cent of the area of the United States, are used in every type of civil engineering as well as by the public generally. The annual distribution by sale of half a million of these maps is an index of their value to the people.

The hot discussion that was waged for years on the question of military versus scientific administration of topographic surveys is in striking contrast with the present concentration of all the topographic mapping under the Geological Survey in those areas where it may best serve the needs of the Army. In 1916 Congress specifically recognized the possibility of greater coöperation of this kind, both in the appropriation made to

the Geological Survey and in a special appropriation made to the War Department. For a number of years indeed special military information had been contributed to the Army by the Survey topographers, but since March 26, 1917, every Geological Survey topographer has worked exclusively on the program of military surveys laid down by the General Staff of the Army, and the places of some of the 44 Survey topographers now in France as engineer officers are filled by 34 other reserve engineer officers detailed by order of the Secretary of War to the Director of the Geological Survey to assist in this military mapping and to receive instruction fitting them in turn for topographic service in France.

The contribution of this civilian service to the military operations in the present emergency forms a fitting conclusion to this review of a century of Government surveys. At present 215 members of the Geological Survey are in uniform, 107 as engineer officers, two of whom are on the staff of the American Commanding General in France. In the war work carried on in the United States the Survey's contribution is by no means limited to military mapping: the geologists are also mobilized for meeting war needs, assisting in developing new sources of the essential war minerals, in speeding up production of mineral products, in collecting information for the purchasing officers both of our own and of the Allied governments, in coöperating with the constructing quartermasters in the location of gravel and sand for structural use and in both general and special examinations of underground water supply and of drainage possibilities at cantonment sites, and in supplying the Navy Department with similar technical data. A special contribution has been the application to aërial surveys of photogrammetric methods developed in the Alaskan topographic work and the perfection of a camera specially adapted to airplane use. The utilization of the Survey's map engraving and printing plant for confidential and urgent work for both the Army and Navy has necessitated postponement of current work for the Geological Survey itself. Throughout the organization the records, the methods, and the personnel which represent the product

of many years of scientific activity are all being utilized; thus is the experience of the past translated into special service in the present crisis.

Notes.

[1] Hess, R. H., Foundations of National Prosperity, p. 100.

[2] Report Nat'l Museum, 1904, pp. 189-733.

[3] Featherstonhaugh, J. D., Am. Geol., 3, 220, 1889.

[4] Whitney, Mineral Wealth of the United States, pp. 248-250.

[5] Foster and Whitney, 31st Cong., 1st session, House Doc. 69, pp. 13-14, 1850.

[6] First Annual Rept. U. S. Geol. Survey, p. 4.

[7] Wheeler, Report 3d Internat'l Geog. Cong., p. 492, 1885.

[8] The views of the writer on ''natural monopolies'' in the Government service are set forth in an address delivered at the centennial celebration of the U. S. Coast and Geodetic Survey, April 5, 1916. (See Science, vol. 43, pp. 659-665, May 12, 1916.)

[9] For correspondence on this subject, see Minnesota Geol. Survey, Eighth Ann. Rept., 1880, p. 173.

[10] Owen, D. D., 30th Cong., 1st sess., Senate Doc. No. 57, p. 7, 1848.

[11] This essential difference between present-day requirements and the needs of earlier generations has been discussed by W. C. Mendenhall, the geologist in charge of the Land Classification Board of the Geological Survey: Proceedings 2d Pan-American Sci. Cong., 1915-16, 3, 761.

VI

ON THE DEVELOPMENT OF VERTEBRATE PALEONTOLOGY

By RICHARD SWANN LULL

Introduction.

UNLIKE its sister science of Invertebrate Paleontology, which has been approached so largely from the viewpoint of stratigraphic geology, that of the vertebrates is essentially a biologic science, having its inception in the masterly work of Cuvier, who is also to be regarded as the founder of comparative anatomy. For long decades, vertebrate paleontology was simply a branch of comparative anatomy or morphology in that it dealt almost exclusively with the form and other peculiarities of fossil bones and teeth, often in a more or less fragmentary condition, very little or no attention being paid to any other system of the creature's anatomy. Distribution both in space and in time was recorded, but the value of vertebrates in stratigraphy was still to be appreciated and has hardly yet come into its own. It is readily seen, therefore, that the two departments of paleontology did not enlist the same workers or even the same type of investigators, for while the two sciences have much in common and should have more, the vertebratist must, above all else, be a morphologist, with a keen appreciation of form, and a mind capable of retaining endless structural details and of visualizing as a whole what may be known only in part. The initial work of the brilliant Cuvier set so high a standard of preparedness and mental equipment that as a consequence, the number of those engaged in vertebrate research has never been large as compared with the workers in some other branches of science, but the results achieved by the few

who have consecrated their research to the fossil vertebrates has been in the main of a high order.

At first, as has been emphasized, this work was largely morphological, dealing both with the individual skeletal elements and later with the bony framework as a whole. Then came the endeavor to clothe the bones with sinews and with flesh—to imagine, in other words, the life-appearance of the ages-departed form—with such of its habits as could be deduced from structure of body, tooth, and limb. Next came the working out of systematic series of vertebrates and their marshalling into species, genera, and larger groups, and much time was thus spent, especially when rapid discovery brought a continual stream of new forms before the systematist, and hence some appreciation of the countless hosts of bygone creatures which peopled the world in the geologic past. This systematic work, however, was based upon the most painstaking morphologic comparisons and so the science was still within the scope of comparative anatomy.

In connection with taxonomic research came increasingly tangible evidence in favor of the law of evolution; investigators turned to the working out of phyletic series showing the actual record of the successive evolutionary changes that the various races had undergone. Coupled with this evolutionary evidence came an increased attention to the sequential occurrence in successive geologic strata, and the stratigraphic distribution of vertebrates became known with greater and greater detail. Then followed the assemblage of faunas, which brought the study of the fossil forms within the realm of historical geology, rather than being the mere phylogeny of a single race, and the value of vertebrate fossils as horizon markers became more and more appreciated by the stratigrapher. They serve to supplement the knowledge gained from the invertebrates, and in this connection are especially valuable in that they often give data concerning continental formations about which invertebrate paleontology is largely silent.

Rise of Vertebrate Paleontology in Europe.

To those who had been nurtured in the belief in a relatively recent creation covering in its entirety a period of

but six days, and occurring but four millenniums before the time of Christ, the appearance of the remains of creatures in the rocks, the like of which no man ever saw alive, must have given scope to the wildest imaginings concerning their origin and significance; for many believed that not only had no new forms been added to the world's fauna since the creation, except possibly by hybridizing, but that none had become extinct save a very few through the agency of human interference. The supposition was, therefore, that such creatures as were thus discovered were still extant in some more remote fastnesses of the world. Thus, our second president, Thomas Jefferson, who wrote one of the first papers on American fossil vertebrates, published in 1798, discussed therein the remains of a huge ground-sloth which has since borne the name *Megalonyx jeffersoni*. Jefferson, however, described the great claws as pertaining to a huge leonine animal which he firmly believed was yet living among the mountains of Virginia.

Cuvier (1769-1832) has been spoken of as the founder of our science. His opportunity lay in the profusion of bones buried in the gypsum deposits of Montmartre within the environs of the city of Paris. Cuvier's studies of these remains, done in the light of his very broad anatomical knowledge, enabled him to prepare the first reconstructions of fossil vertebrates ever attempted and to bring before the eyes of his contemporaries a world peopled with forms which were utterly extinct. That these creatures were no longer living, none was a better judge than Cuvier, for his prominence was such that material was sent him from all parts of the world, to which must be added that which he saw in his visits to the various museums of Europe. He felt it safe, therefore, to affirm the unlikelihood of any further discovery of unknown forms among the great mammals of the present fauna of our globe, and few indeed have been the additions since his day. To Cuvier is due not alone the masterly contribution to the sister sciences of comparative anatomy and vertebrate paleontology—the Ossements Fossiles (1812)—but he also announced the presence in continental strata of a series of faunas which showed a gradual organic improvement from the earliest

such assemblage to the most modern, an idea of the most fundamental importance and one with which he is rarely credited. He believed in the sudden and complete extinction of faunas, and the facts then known were in accord with this idea, as no common genera nor transitional forms connected the creatures of the Paris gypsum with the mastodons, elephants, and hippopotami which the later strata disclosed. It is not remarkable, therefore, that Cuvier advanced his theory of catastrophism to account for these extinctions. He should not, however, according to Depéret, be credited with the idea of successive re-creations, such as that held by D'Orbigny and others, but of repopulation by immigration from some area which the catastrophe, be it flood or other destructive agency, failed to reach.

Cuvier was followed in Europe by a number of illustrious men, none of whom, however, with the exception of Sir Richard Owen, possessed his breadth of knowledge of comparative anatomy upon which to base their researches among the prehistoric. The more notable of them may be enumerated before going on to a discussion of the American contributions to the science.

They were, first, Louis Agassiz, a pupil of Cuvier and later a resident of America, whose researches on the fossil fishes of Europe are a monumental work, the result of ten years of investigation in all of the larger museums of that continent, and which appeared in 1833-43, while he was yet a young man. The fishes were practically the only fossil vertebrates to come within the scope of his investigations, for his later time was consumed in the study of glaciers and of recent marine zoology. Another student of these most primitive vertebrates who left an enduring monument was Johannes Müller. Huxley, Traquair, and Jaekel also did masterly work upon this group, while Smith Woodward of the British Museum is considered the highest living authority upon fossil fishes.

Of the Amphibia, the most famous foreign students were Brongniart, Jaeger, Burmeister, Von Meyer, and Owen, although Owen's claim to eminence lies rather in the investigations of fossil reptiles which he began in 1839 and continued over a period of fifty years of remarkable achievement. Not only did he describe the

dinosaurs of Great Britain in a series of splendidly illustrated monographs, but extended his researches to the curious reptilian forms from the Karroo formation of South Africa. It was to him, moreover, that the establishment of the true position of the famous *Archæopteryx* as the earliest known bird and not a reptile is due. Von Meyer also enriched the literature of fossil reptiles, discussing exhaustively those occurring in Germany, while Huxley's classic work on the crocodiles as well as on dinosaurs, and the labors of Buckland, Fraas, Koken, Von Huene, Gaudry, Hulke, Seeley, and Lydekker have added immensely to our knowledge of the group.

Of the birds, which at best are rare as fossils, our knowledge, especially of the huge flightless moas, is due largely again to Owen, and his realization of the systematic position of *Archæopteryx* has already been mentioned.

The mammals were, perhaps, the most prolific source of paleontological research during the nineteenth century, for, as Zittel has said, Cuvier's famous investigations on the fossil bones, mentioned above, not only contain the principles of comparative osteology, but also show in a manner which has never been surpassed how fossil vertebrates ought to be studied, and what are the broad inductions which may be drawn from a series of methodical observations. Such was Cuvier's influence that until Darwin began to interest himself in mammalian paleontology the study of these forms was conducted entirely along the lines indicated by the French savant. This was seen in a large work, Osteology of Recent and Fossil Mammalia, by De Blainville, which, although not up to the standard set by the master, is nevertheless a notable contribution, as was also the Osteology prepared by Pander and D'Alton. A summary of the knowledge of the fossil Mammalia up to the year 1847 is contained in Giebel's Fauna der Vorwelt, and Lydekker has done for the mammals in the British Museum what Smith Woodward did for the fishes, producing vastly more than the mere catalogue which the title implies.

The first work wherein the fossil mammals were treated genealogically was Gaudry's Enchaînements du Monde Animal, written in 1878. Other work on the

fossil Mammalia was done by Kaup, who described those from the Mainz basin and from Epplesheim near Worms whence came one of the most famous of prehistoric horses, the *Hipparion;* this horse, together with the remarkable proboscidean *Dinotherium,* was described by Von Meyer. One of the most remarkable discoveries, ranking in importance, perhaps, next to Montmartre, was that of the Pliocene fauna of Pikermi near Athens, Greece, first made known through the publications of A. Wagner of Munich and later, and much more extensively, through that of Gaudry (1862-1867). H. von Meyer was Germany's best authority on fossil Mammalia. After his death the work was carried on by Quenstedt, Oscar Fraas, Schlosser, Koken, and Pohlig, among others.

In France, rich deposits of fossil mammals were discovered in the Department of Puy-de-Dôme, the Rhone basin, Sansan, Quercy, and near Rheims. These were described by a number of writers, notably Croizet and Jobert, Pomel, Lartet, Filhol, and Lemoine.

Rütimeyer of Bâle was one of the most famous European writers on mammalian paleontology, and his researches were both comprehensive and clothed in such form as to give them a high place in paleontological literature. He studied comparatively the teeth of ungulates, discussed the genealogy of mammals, and the relationships of those of the Old and New Worlds. He was an exponent of the law of evolution as set forth by Darwin, and his "genealogical trees of the Mammalia show a complete knowledge of all the data concerning the different members in the succession, and are amongst the finest results hitherto obtained by means of strict scientific methods of investigation" (Zittel, History of Geology and Palæontology, 1901). The mammals of the Swiss Eocene have been studied in much detail by Stehlin.

For Great Britain, the most notable contributors were Buckland in his Reliquiæ Diluvianæ; Falconer, co-author with Cautley on the Tertiary mammals of India; Charles Murchison, who wrote on rhinoceroses and proboscideans; and more recently Bush, Flower, Lydekker, Boyd Dawkins, L. Adams, and C. W. Andrews. But by far the most commanding figure of all was Sir Richard Owen,

who for half a century stood without a peer as the greatest of authorities on fossil mammals. It was the Natural History of the British Fossil Mammals and Birds, published in 1846, that established Sir Richard's reputation.

Russia has produced much mammalian material, especially from the Tertiary of Odessa and Bessarabia, and from the Quaternary of northern Russia and Siberia. These have been described mainly by J. F. Brandt, A. von Nordmann, but especially by Mme. M. Pavlow of Moscow.

Forsyth-Major discovered in 1887 a fauna contemporaneous with that of Pikermi in the Island of Samos in the Mediterranean.

One of the most remarkable recent discoveries of fossil localities was that announced in 1901 by Mr. Hugh J. L. Beadnell of the Geological Survey of Egypt and Doctor C. W. Andrews of the British Museum of London, of numerous land and sea mammals of Upper Eocene and Lower Oligocene age in northern Egypt. The exposures lay about 80 miles southwest of Cairo in the Fayûm district and are the sediments of an ancient Tertiary lake, a relic of which, Birket-el-Qurun, yet remains. These beds contained ancient Hyracoidea, Sirenia, and Zeuglodontia, but above all, ancestral Proboscidea which, together with those known elsewhere, enabled Andrews to demonstrate the origin and evolutionary features of this most remarkable group of beasts. This discovery in the Fayûm lends color to the belief that Africa may have been the ancestral home of at least five of the mammalian orders, those mentioned above, together with the Embrithopoda, a group unknown elsewhere. This theory had been advanced independently by Tullberg, Stehlin, and Osborn, before the discovery in Egypt.

Another European worker of pre-eminence who wrote more broadly than the faunal studies mentioned above was W. Kowalewsky. He discussed especially the evolutionary changes of feet and teeth in ungulates, a line of research afterward developed in greater detail by the Americans, Cope and Osborn.

South America has yielded series of rich faunas which have been exploited by the great Argentinian, Florentino

Ameghino, and by the Europeans, Owen, Gervais, Huxley, Von Meyer, and more recently by Burmeister and Lydekker. Later exploration and research by Hatcher and Scott of North America will be discussed further on in this paper.

Vertebrate Paleontology in America.

Early Writers.—Having thus summarized paleontological progress in the Old World, we can turn to a consideration of the work done in the New, especially in the United States, because while the Old World investigation has been invaluable, a science of vertebrate paleontology, very complete both as to its zoological and geological scope and in the extent and value of published results, could be built exclusively upon the discoveries and researches made by Americans. The science of vertebrate paleontology may be said to have had its beginnings in North America with the activities of Thomas Jefferson, who, like Franklin, felt so strong an interest in scientific pursuits that even the graver duties of the highest office in the gift of the American people could not deter him from them. When in 1797 Jefferson came to be inaugurated as vice-president of the United States, he brought with him to Philadelphia not only his manuscript but the actual fossil bones upon which it was based. Again in 1801 he was greatly interested in the Shawangunk mastodon, despite heavy cares of state, and in 1808 made part of the executive mansion in Washington serve as a paleontological laboratory, displaying therein for study the bones of proboscideans and their contemporaries which the Big Bone Lick of Kentucky had produced. Jefferson's work would not, perhaps, have been epoch-making were it not for its unique chronological position in the annals of the science.

Jefferson was followed by another man—this time one whose diverging lines of interest led him not into the realm of political service, but of art, for Rembrandt Peale possessed an enviable reputation among the early painters of America. Peale published in 1802 an account of the skeleton of the "mammoth," really the mastodon, *M. americanus,* speaking of it as a nondescript carnivor-

ous animal of immense size found in America. It was because of the form of the molar teeth that Peale said of it: "If this animal was indeed carnivorous, which I believe cannot be doubted, though we may as philosophers regret it, as men we cannot but thank Heaven that its whole generation is probably extinct."

With the work of these men as a beginning, it is not strange that the more conspicuous Pleistocene fossils of the East should have attracted the attention of many subsequent writers in the first part of the nineteenth century, nor that the early papers to appear in the Journal should pertain to proboscideans or to the huge edentate ground-sloths and the aberrant zeuglodons whose bones frequently came to light. Therefore a number of men such as Koch, both Sillimans, J. C. Warren, and others made these forms their chief concern.

Fossil Footprints.—Among the early writers who concerned themselves with these greater fossils was Edward Hitchcock, sometime president of Amherst College, and a geologist of high repute among his contemporaries. Hitchcock is, however, better and more widely known as the pioneer worker on a series of phenomena displayed as in no other place in the region in which he made his home. These are fossil footprints impressed upon the Triassic rocks of the Connecticut valley. It was in the Journal for the year 1836 (**29**, 307-340) that Hitchcock first called attention to the footmarks, although they had been known and discussed popularly for a number of years previous. James Deane, of Greenfield, was perhaps the first to appreciate the scientific interest of these phenomena, but deeming his own qualifications insufficient properly to describe them, he brought them to the attention of Hitchcock, and the interest of the latter never waned until his death in 1864. Hitchcock wrote paper after paper, publishing many of them in the Journal, again in his Final Report on the Geology of Massachusetts (1841), and later in quarto works, one in the Memoirs of the American Academy of Arts and Sciences and the two others under the authority of the Commonwealth, the Ichnology in 1858, and the Supplement in 1865, the last being a posthumous work edited by his son, Charles H. Hitchcock.

Hitchcock's conception of the track-makers was more or less imperfect because of the fact that for a long time but a few fragmentary osseous remains were known, either directly or indirectly associated with the tracks, while on the other hand the bird-like character of many of the latter and the discovery of huge flightless birds elsewhere on the globe suggested a very close analogy if not a direct relationship. Hence "bird tracks" they were straightway called, a designation which it has been difficult to remove, even though in 1843 Owen called attention to the need of caution in assuming the existence of so highly organized birds at so early a period, especially when large *reptiles* were known which might readily form very similar tracks. The footprints are now believed to be very largely of dinosaurian origin, and dinosaurs whose feet corresponded in every detail with the footprints have actually come to light within the same geologic and geographic limitations. This of course refers to the bipedal, functionally three-toed tracks. Of the makers of certain of the obscurer of the quadrupedal trails we are as much in the dark to-day as were the first discoverers of a century ago, so far as demonstrable proof is concerned. We assume, however, that they were the tracks of amphibia and reptiles, beyond which we may not go with certainty.

Agassiz, writing in 1865 (Geological Sketches), says:

> "To sum up my opinion respecting these footmarks, I believe that they were made by animals of a prophetic type, belonging to the class of reptiles, and exhibiting many synthetic characters. The more closely we study past creations, the more impressive and significant do the synthetic types, presenting features of the higher classes under the guise of the lower ones, become. They hold the promise of the future. As the opening overture of an opera contains all the musical elements to be therein developed, so this living prelude of the creative work comprises all the organic elements to be successively developed in the course of time."

Of those whose work was contemporaneous with that of Hitchcock, but one, W. C. Redfield, wrote on Triassic phenomena, and he concerned himself mainly with the fossil fishes of that time, his first paper on this subject

appearing in 1837 in the Journal (**34**, 201), and the last twenty years later.

Paleozoic Vertebrates.—Later the vertebrates of the Paleozoic began to attract attention, footprints from Pennsylvania being described by Isaac Lea, beginning in 1849, a notice of his first paper appearing in the Journal for that year (**9**, 124). Several papers followed on the reptile *Clepsysaurus.* Alfred King also wrote on the Carboniferous ichnites, his work slightly antedating that of Lea, but being less authoritative.

But by far the most illuminating of the mid-century writers on Paleozoic vertebrates was Sir William Dawson, a very large proportion of whose numerous papers relate to the Coal Measures of Nova Scotia and their contained plant and animal remains. In 1853 appeared Dawson's first announcement, written in collaboration with Sir Charles Lyell, of the finding of the bones of vertebrates within the base of an upright fossil tree trunk at South Joggins. These bones were identified by Owen and Wyman as pertaining to a reptilian or amphibian to which the name *Dendrerpeton acadianum* was given. Following this were several papers published in the Quarterly Journal of the Geological Society, London, describing more vertebrates and associated terrestrial molluscs. In 1863 Dawson summarized his discoveries in the Journal (**36**, 430-432) under the title of "Air-breathers of the Coal Period," a paper which was expanded and published under the same title in the Canadian Naturalist and Geologist for the same year. Dawson also printed in the same volume the first account of reptilian(?) footprints from the coal. Thus from time to time there emanated from his prolific pen the account of further discoveries, both in bones and footprints, his final synopsis of the air-breathing animals of the Paleozoic of Canada appearing in 1895. The only other group of vertebrates which claimed his attention were certain whales, on which he occasionally wrote.

Fishes.—The fossil fishes from the Devonian of Ohio found their first exponent in J. S. Newberry, appointed chief geologist of the second geological survey of Ohio, which was established in 1869. These fishes from the Devonian shales belonged for the greater part to the

curious group of armored placoderms, the remains of which consist very largely of armor plates with little or no traces of internal skeleton. There was also found in association a shark, *Cladoselache,* of such marvelous preservation that from some of the Newberry specimens now in the American Museum of Natural History, New York, Bashford Dean has demonstrated the histology of muscle and visceral organs, in addition to the very complete skeletal remains.

Newberry's work on these forms, begun in 1868, has been carried to further completion by Bashford Dean and his pupil L. Hussakof, as well as by C. R. Eastman. Newberry's other paleontological work was with the Carboniferous fishes of Ohio, the Carboniferous and Triassic fishes of the region from Sante Fé to the Grand and Green rivers, Colorado, and on the fishes and plants of the Newark system of the Connecticut valley and New Jersey. He also discussed certain mastodon and mammoth remains, and those of the peccary of Ohio, *Dicotyles.*

Joseph Leidy (1823-1891).

We now come to a consideration of the work of Joseph Leidy, one of the three great pioneers in American vertebrate paleontology, for if we disregard the work of Hitchcock and others on the fossil footprints, few of the results thus far obtained were based upon the fruits of organized research. Leidy began his publication in 1847 and continued to issue papers and books from time to time until the year 1892, having published no fewer than 219 paleontological titles, and 553 all told. His earlier paleontological researches were exclusively on the Mammalia, which were then coming in from the newly discovered fossil localities of the West. The discovery of these forms, one of the most notable events in the history of our science, will bear re-telling.

The first announcement was made in 1847, when Hiram A. Prout of St. Louis published in the Journal (**3**, 248-250) the description of the maxillary bone of "*Palæotherium*" (=*Titanotherium proutii*) from near White River, Nebraska. This at once drew the attention of geologists and paleontologists to the Bad Lands, or

Mauvaises Terres, which were to prove so highly productive of fossil forms. About the same time S. D. Culbertson of Chambersburg, Pennsylvania, submitted to the Academy of Natural Sciences at Philadelphia some fossils sent to him from Nebraska by Alexander Culbertson. These were afterward described by Leidy in the Proceedings of the Academy, together with the paleotheroid jaw, in addition to which three other collections which had been made were also placed at his disposal for study.

This aroused the interest of Doctor Spencer F. Baird of the Smithsonian Institution, who sent T. A. Culbertson to the Bad Lands to make further collections. The latter was successful in securing a valuable series of mammalian and chelonian remains. These, together with other specimens from the same locality, were sent to Leidy, for, as Baird remarked, Leidy, although only thirty years of age, was the only anatomist in the United States qualified to determine their nature. The outcome of Leidy's study of this material was "The Ancient Fauna of Nebraska," published in 1853, and constituting the most brilliant work which up to that time American paleontology had produced. Leidy's determinations, which are in the main correct, are the more remarkable when it is realized that he had little recent osteological material for comparative study. The forms thus described by him were new to science, of a more generalized character than those now living, and yet their distinguished describer recognized, either at that time or a little later, their true relationship to the modern types. The extent of Leidy's anatomical knowledge was almost Cuvierian, and Cuvier-like he established the fact of the presence of the rhinoceroses, then unheard of in the American fauna, from a few small fragments of molar teeth, an opinion shortly to be fully sustained through the finding of complete molars and the entire skull of the *same individual animal.*

Leidy next turned his attention to the huge edentates, which he studied exhaustively, publishing his results in the form of a memoir in 1855, two years after the appearance of the "Ancient Fauna."

Extinct fishes of the Devonian of Illinois and Missouri and the Devonian and Carboniferous of Pennsylvania

were made the subjects of his next researches, after which he described the peccaries of Ohio, and later, in a much larger and most important work, the Cretaceous reptiles of the United States (1865). Most of the fossils discussed in this last work are from the New Jersey Cretaceous marls and of them the most notable was the herbivorous dinosaur *Hadrosaurus*, the structure and habits of which, together with its affinities with the Old World iguanodons, Leidy described in detail. From Leidy's descriptions and with his aid, Waterhouse Hawkins was enabled to restore a replica of the skeleton in a remarkably efficient way. This restoration for a long time graced the museum of the Philadelphia Academy of Natural Sciences and there was a plaster replica of it in the United States National Museum. These, together with plaster replicas of *Iguanodon* from the Royal College of Surgeons in London, gave to Americans their first real conceptions of members of this most remarkable group. The associated fossils from the New Jersey marls were chiefly crocodiles and turtles.

From 1853 to 1866 F. V. Hayden was carrying on a series of most energetic explorations in the West, especially in Nebraska and Dakota as then delimited, returning from each trip laden with fossils which were given to Leidy for determination. The results appeared in 1869 in Leidy's Extinct Mammalian Fauna of Dakota and Nebraska, published as volume 7 of the Journal of the Philadelphia Academy. In this large volume no fewer than seventy genera and numerous species of forms, many of them new to science, were described, representing many of the principal mammalian orders; horses were, however, especially conspicuous. This last group led Leidy to the conclusion, afterward emphasized by Huxley, that North America was the home of the horse in geologic time, there being here a greater representation of different species than in any recent fauna of the world. Leidy's interest in the horses, for the forwarding of which he made a large collection of recent material, extended over many years, as his first paper on the subject bears the date of 1847, the last that of 1890.

Next came the discovery of Eocene material from the vicinity of Fort Bridger, Wyoming, geologically older

than the Nebraska and Dakota formations. This, together with specimens from the Green River and Sweetwater River deposits of Wyoming and the John Day River (Oligocene) of Oregon, was also referred to Leidy, and added yet more to the list of newly discovered species with which he had already become familiar in his earlier researches. The results of this study were published by the Hayden Survey in 1873, under the title "Contributions to the Extinct Vertebrate Fauna of the Western Territories." This was the last of Leidy's major works, but he continued up to the time of his death to report to the Academy concerning the various fossil forms that were submitted to him for identification. Of such reports the most important was one on the fossils of the phosphate beds of South Carolina, published in the Journal of the Academy in 1887.

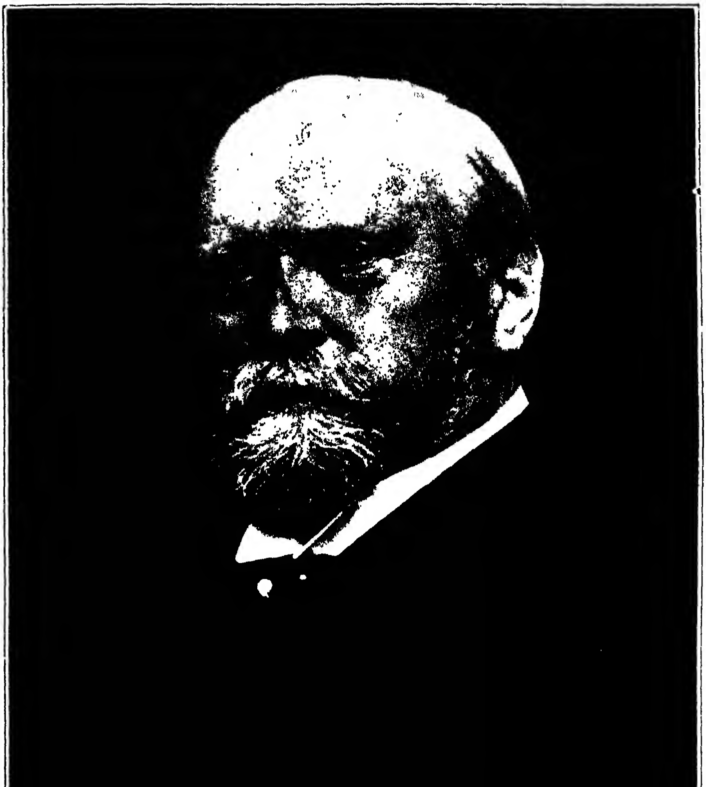
As a paleontologist, Leidy ranks with Cope and Marsh high among those who enriched the American literature of the subject, but it must be remembered that this was but a single aspect of his many-sided scientific career, for he made many contributions of high order to botany, zoology, and general and comparative anatomy as well, nor did his knowledge and usefulness as an instructor of his fellow men keep within the limitations of these subjects.

Othniel Charles Marsh (1831-1899).

The sixth decade of the nineteenth century saw the beginning of the labors of several paleontologists who, like Leidy, were destined to raise the science of fossil vertebrates in America to the level of attainment of the Old World. They were, among others, Othniel Charles Marsh and Edward Drinker Cope. Of these the names of Marsh and Cope are linked together by the brilliance of their attainments, their contemporaneity, and the rivalry which the similarity of their pursuits unfortunately engendered. Marsh produced his first paleontological paper in 1862 (**33**, 278), Cope in 1864, but the latter died first, so that his life of research was shorter.

To Professor Marsh should be given credit for the first organized expedition designed exclusively for the collection of vertebrate remains, the results of which con-

tain so much material that it has not yet entirely seen the light of scientific exposition. Marsh's first trip to the West was in 1868, the first formal expedition being organized two years later. These expeditions, of which there were four, were privately financed except for the material and military escort furnished by the United States Government, and consisted of a personnel drawn entirely from the graduate or undergraduate body of Yale University. These parties explored Kansas, Nebraska, Wyoming, Utah, and Oregon, and returned laden with material from the Cretaceous and Tertiary formations of the West. Some of this is of necessity somewhat fragmentary, but type after type was secured which, with his exhaustive knowledge of comparative anatomy, enabled Marsh to announce discovery after discovery of species, genera, families, and even orders of mammals, birds, and reptiles which were unknown to science. The year 1873 saw the last of the student expeditions, and thereafter until the close of his life the work of collecting was done under Marsh's supervision, but by paid explorers, many of whom had been his scouts and guides in the formal expeditions or had been especially trained by him in the East. In 1882, after fourteen years of the experience thus gained, Marsh was appointed vertebrate paleontologist to the United States Geological Survey, which relieved him in part of the personal expense connected with the collecting, although up to within a short time of his death his own fortune was very largely spent in enlarging his collections. After his connection with the Survey was established, Marsh had two main purposes in view in making the collections: (1) to determine the geological horizon of each locality where a large series of vertebrate fossils was found, and (2) to secure from these localities large collections of the more important forms sufficiently extensive to reveal, if possible, the life histories of each. Marsh believed that the material thus secured would serve as key, or diagnostic fossils to all horizons of our western geology above the Paleozoic, a belief in which he was in advance of his time, for few of his contemporaries appreciated the value of vertebrates as horizon markers. The result of the fulfilment of his second purpose saw the accumulation of



O. C. Marsh

huge collections from all horizons above the Triassic and some Paleozoic and Triassic as well. These contained some very remarkable series, each of which Marsh hoped to make the basis of an elaborate monograph to be published under the auspices of the Survey. One can visualize the scope of his ambitions by the fact that no fewer than twenty-seven projected quarto volumes, to contain at least 850 lithographic plates, were listed by him in 1877. These covered, among other groups, the toothed birds (Odontornithes), Dinocerata, horses, brontotheres, pterodactyls, mosasaurs and plesiosaurs, monkeys, carnivores, perissodactyls and artiodactyls, crocodiles, lizards, dinosaurs, various birds, proboscideans, edentates and marsupials, brain evolution, and the Connecticut Valley footprints. Much was done towards the preparation of these memoirs, as evidenced by the long list of preliminary papers, admirably illustrated by woodcuts which were to form the text figures of the memoirs, which appeared with great regularity in the pages of the Journal for a period of thirty years. Of the actual memoirs, however, but two had been published at the time of Marsh's death in 1899—the Odontornithes in 1880 and the Dinocerata in 1884. One must not overlook, however, the epoch-making Dinosaurs of North America, which was published by the Survey in 1896, although it was not in the form nor had it the scope of the proposed monographs. This was not due to lack of application, for Professor Marsh was an indefatigable worker, but rather to the fact that the program was of such magnitude as to necessitate a patriarchal life span for its consummation. As it is, Professor Marsh's fame rests first upon his ability and intrepidity as a collector, ready himself to brave the very certain hardships and dangers which beset the field paleontologist in the pioneer days, and also by his judgment and command of men to secure the very adequate services of others and so to direct their endeavors that the results were of the highest value. The material witness to Marsh's skill as a collector lies in the collections of the Peabody Museum at Yale and in the Marsh collection at the United States National Museum, the latter secured through the funds of the United States Geological Survey. Together they consti-

tute what is possibly the greatest collection of fossil vertebrates in America, if not in the world; individually, they are second only to that of the American Museum in New York City, the result of the combined labors of Osborn and Cope and their very able corps of assistants.

As a scientist Marsh possessed in large measure that wide knowledge of comparative anatomy so necessary to the vertebrate paleontologist, and as a consequence was not only able to recognize affinities and classify unerringly, but also to recognize the salient diagnostic features of the form before him and in few words so to describe them as to render the recognition of the species by another worker relatively easy. The publication of hundreds of these specific diagnoses in the Journal constitutes a very large and valuable part of that periodical's contribution to the advancement of our science. Marsh's method of indicating forms by so brief a statement leaves much to be done, however, in the way of further description of his types, which in many instances were but partially prepared.

Yet another important service which Marsh rendered to science was the restoration of the creatures as a whole, made with the most painstaking care and precision through assembling the drawings of the individual bones. These restorations have become classic, embracing as they did a score or more of forms, of beast, bird, and reptile. They also were published first in the Journal, although they have subsequently been reproduced in text-books and other works the world over. Part of Marsh's popular reputation, at least, which was second to that of no other American in his line, was due to his skill in attaining publicity, for his papers, of whatever extent, were carefully and methodically sent to correspondents in the uttermost parts of the earth, and thus the Marsh collection has reflected the fame of its maker.

Edward Drinker Cope (1840-1897).

The third great name in American vertebrate paleontology, that of Edward Drinker Cope, stands out in sharp contrast with the other two, although in the range of his interests he was probably more nearly comparable with

Leidy than with Marsh. The beginning of Cope's scientific labors dates from 1859, the year made famous in the annals of science by the appearance of Darwin's Origin of Species. It is not surprising, therefore, that matters evolutional should have interested him to the very end of his career. Cope was not merely a paleontologist, but was interested in recent forms, especially the three lower classes of vertebrates, to such an extent that his work therewith is highly authoritative and in some respects epoch-making. Thirty-eight years of almost continual toil were his, and the mere mass of his literary productions is prodigious, especially when one realizes that, unlike those of a writer of fiction, they were based on painstaking research and philosophical thought. The greater part of Cope's life was spent in or near Philadelphia except for his western explorations, and he is best known as professor of geology and paleontology in the University of Pennsylvania, although he served other institutions as well.

Cope's early work was among the amphibia and reptiles, his first paleontological paper, the description of *Amphibamus grandiceps*, appearing in 1865. This year he also began his studies of the mammals, especially the Cetacea, both living and extinct, from the Atlantic seaboard. The next year saw the beginning of his work on the material from the Cretaceous marls of New Jersey, describing therefrom one of the first carnivorous dinosaurs, *Lælaps*, to be discovered in America. In 1868 Cope began to describe the vertebrates from the Kansas chalk and three years later made his first exploration of these beds. This led to his connection with the United States Geological Survey of the Territories under Hayden, and to continued exploration of Wyoming and Colorado in 1872 and 1873. The material thus gained, consisting of fishes, mosasaurs, dinosaurs, and other reptiles, was described in the Transactions of the American Philosophical Society as well as in the Survey Bulletins. In 1875 these results were summarized in a large quarto volume entitled "Vertebrata of the Cretaceous formations of the West." Subsequent summers were spent in further exploration of the Bridger, Washakie, and Wasatch formations of Wyoming, the Puerco

and Torrejon of New Mexico, and the Judith River of Montana. The material gathered in New Mexico proved particularly valuable, and led to the publication in 1877 of another notable volume entitled "Report upon the Extinct Vertebrata obtained in New Mexico by Parties of the Expedition of 1874."

Material was now accumulating so fast as to necessitate the concentration of Cope's own time on research, so that, while he continued to make brief journeys to the West, the real work of exploration was delegated to Charles H. Sternberg and J. L. Wortman, both of whom became subsequently very well known, the former as a collector whose active service has not yet ceased, the latter as an explorer and later an investigator of extremely high promise.

As early as 1865, Cope began no fewer than five separate lines of research which he pursued concurrently for the remainder of his career. On the fishes, he became a high authority in the larger classification, owing to his researches into their phylogeny, for which a knowledge of extinct forms is imperative. On amphibia, he wrote more voluminously than any other naturalist, discussing not only the morphology but the paleontology and taxonomy as well. In this connection must be mentioned not only Cope's exploration and collections in the Permian of Ohio and Illinois, but especially the remains from the Texas Permian, first received in 1877, upon which some of his most brilliant results were based; these of course included reptilian as well as amphibian material. His third line of research, the Reptilia, is in part included in the foregoing, but also embraced the reptiles of the Bridger and other Tertiary deposits, those of the Kansas Cretaceous, and the Cretaceous dinosaurs.

Up to 1868 Leidy alone was engaged in research in the West, but that year saw the simultaneous entrance of Marsh and Cope into this new field of research, and their exploration and descriptions of similar regions and forms soon led to a rivalry which in turn developed into a most unfortunate series of controversies, mainly over the subject of priority. This resulted in a permanent rupture of friendship and the division of American workers into two opposing camps to the detriment of the progress of

our science. This breach has now been happily healed, and for a number of years the degree of mutual good will and aid on the part of our workers has been of the highest sort.

The extent of the western fossil area, and particularly the explorations of three of Cope's aids, Wortman in the Big Horn and Wasatch basins, Baldwin in the Puerco of New Mexico, and Cummins in the Permian of Texas, gave him so fruitful a field of endeavor that the occasion for jealous rivalry was largely removed. The most manifest result of Cope's western work was the publication in 1883 of his Vertebrata of the Tertiary Formations of the West, which formed volume **3** of the quarto publications of the Hayden Survey. This huge book contains more than 1000 pages and 80 plates and has been facetiously called "Cope's Bible."

Cope's philosophical contributions, which covered the domains of evolution, psychology, ethics, and metaphysics, began in 1868 with his paper on The Origin of Genera. In evolution he was a follower of Lamarck, and as such, with Hyatt, Ryder, and Packard, was one of the founders of the so-called Neo-Lamarckian School in America. Cope's principal contribution, set forth in his Factors of Organic Evolution, is the idea of kinetogenesis or mechanical genesis, the principle that all structures are the direct outcome of the stresses and strains to which the organism is subjected. Weismann's forcible attack on the transmission theory did not shake Cope's faith in these doctrines, for he claimed that the paleontological evidence for the inheritance of such characters as are apparently the result of *individual* modification was too strong to be refuted. Cope was more like Lamarck than any other naturalist in his mental make-up as well as his ideas. He was also, like Haeckel, given to working out the phylogeny of whatever type lay before him, and in many instances arrived marvellously near the truth as we now see it.

Associated for a while with A. S. Packard, Cope soon became chief editor and proprietor of the American Naturalist, which was for many years his main means of publication and thus served our science in a way comparable to the Journal. As Osborn says by way of summation:

"Cope is not to be thought of merely as a specialist in Paleontology. After Huxley he was the last representative of the old broad-gauge school of anatomists and is only to be compared with members of that school. His life-work bears marks of great genius, of solid and accurate observation, and at times of inaccuracy due to bad logic or haste and overpressure of work. . . . As a comparative anatomist he ranks both in the range and effectiveness of his knowledge and his ideas with Cuvier and Owen. . . . As a natural philosopher, while far less logical than Huxley, he was more creative and constructive, his metaphysics ending in theism rather than agnosticism."

1870-1880.

The seventh decade was productive of comparatively few great names in the history of our science, but two, J. A. Ryder and Samuel W. Williston, being notable contributors. The former produced but few papers and those between 1877 and 1892, yet they were of note and such was their influence that he is named with Hyatt, Packard, and Cope as one of the founders of the Neo-Lamarckian School of evolutionists in America. Ryder was a particular friend and a colleague of Cope, as they were both concerned with the back-boned animals, while the other two were invertebratists. Ryder wrote on mechanical genesis of tooth forms and on scales of fishes, also on the morphology and evolution of the tails of fishes, cetaceans, and sirenians, and of the other fins of aquatic types. He did, on the other hand, practically no systematic or descriptive work.

Williston, on the contrary, has had a long and varied career as an investigator and as an educator. Trained at Yale, he prepared for medicine, and much of his teaching has been of human anatomy, both at Yale and at the University of Kansas where he served for a number of years as dean of the Medical School. He is also a student of flies, and as such not only the foremost but indeed almost the only dipterologist in the United States. But it is with his work as a vertebrate paleontologist that we are chiefly concerned, and here again he stands among the foremost. His initial work and training in this department of science were with Marsh, for whom he spent many months in field work, collecting largely in the Niobrara Cretaceous of Kansas. He did, however, no

research while with Marsh, owing to the latter's disinclination to foster such work on the part of his associates. Williston began his publications in 1878 and has continued them until the present, working mainly with Cretaceous mosasaurs, plesiosaurs, and pterodactyls. Of late, since his transference to the University of Chicago, where as professor of paleontology and director of the Walker Museum he has served since 1902, his interest has lain mainly among the Paleozoic reptiles and amphibia. Williston's more notable works are American Permian Vertebrates and Water Reptiles of the Past and Present, wherein he sets forth his views of the phylogenesis and taxonomy of the reptilian class. He is at present at work on the evolution of the reptiles, a volume which is eagerly awaited by his colleagues. It is in morphology that Williston's greatest strength lies and some of his most effective work on the mosasaurs has appeared in the Journal.

1880-1900.

The next decade, that of 1880-1890, saw a number of notable additions to the workers in vertebrate paleontology: Henry F. Osborn, W. B. Scott, R. W. Shufeldt, J. L. Wortman, George Baur, F. A. Lucas, and F. W. True. Shufeldt is our highest authority on the osteology of birds, both recent and extinct, having recently described all of the extinct forms contained in the Marsh collection; True wrote of Cetacea; Lucas of marine and Pleistocene mammals and birds, and has also written popular books on prehistoric life. Lucas's greatest service, however, lies in the museums, where he has manifested a genius second to none in the installation of mute evidences of living and past organisms. Wortman was for a time associated with Cope, later with Osborn in the American Museum, again at the Carnegie Museum at Pittsburgh, and finally at Yale in research on the Bridger Eocene portion of the Marsh collection. His work has been chiefly the perfection of field methods in vertebrate paleontology, and as a special investigator of Tertiary Mammalia, treating the latter largely from the morphologic and taxonomic standpoints. Wortman's Yale results on the carnivores and primates of the Eocene,

as yet unfinished, were published in the Journal in 1901-1904.

William B. Scott is a graduate of Princeton, and has spent thirty-four years in her service as Blair Professor of Geology and Paleontology. His first publication, in 1878, issued in conjunction with Osborn and Speir, described material collected by them in the Eocene formations of the West, and since that time Scott's research has been entirely with the mammals, on which he is one of our highest authorities. His most notable works have been a History of Land Mammals of the Western Hemisphere, 1913, and the results of the Patagonian expeditions by Hatcher, which are published in a quarto series in conjunction with W. J. Sinclair, although they are the authors of separate volumes, Scott's work being mainly on the carnivores and edentates of the Santa Cruz formation. It is as a systematist in research and as an educator that Scott has attained his highest usefulness.

The man who, next to the three pioneers, has attained the highest reputation in vertebrate paleontologic research, is Henry Fairfield Osborn. Graduate of Princeton in the same class that produced Scott, Osborn served for a time as professor of comparative anatomy in that institution, and in 1891 was called to New York to organize the department of zoology in Columbia University and that of vertebrate paleontology in the American Museum of Natural History. He had, early in his career, gone west in company with Professor Scott, and had collected material from the Eocene formation of Wyoming, upon which they based their first joint paper in 1878, Osborn's first independent production, a memoir on two genera of Dinocerata, appearing in 1881. A number of papers followed, on the Mesozoic Mammalia, on Cope's tritubercular theory, and on certain apparent evidences for the transmission of acquired characters. It was, however, with his acceptance of the New York responsibilities, especially at the American Museum, that Osborn's most significant work began. Aided first by Wortman and Earle, later by W. D. Matthew and others, he has built up the greatest and most complete collection of fossil vertebrates extant; its value, however, was largely enhanced through the purchase of the

private collection of Professor Cope, which of course included a large number of types. The American Museum collection thus contains not only a vast series of representative specimens from every class and order of vertebrates, secured by purchase or expedition from nearly all the great localities of the world, but an exhibition series of skulls and partial and entire skeletons and restorations which no other institution can hope to equal. Based upon this wonderful material is a large amount of research, filling many volumes, published for the greater part in the bulletin and memoirs of the Museum. This research is not only the product of the staff, including Walter Granger, Barnum Brown, W. D. Matthew, and W. K. Gregory, but also of a number of other American and some foreign paleontologists as well.

Professor Osborn's own work has been voluminous, his bibliography from 1877 to 1916 containing no fewer than 441 titles, ranging over the fields of paleontology,--which of course includes the greater number—geology, correlation and paleogeography, evolutionary principles exemplified in the Mammalia, man, neurology and embryology, biographies, and the theory of education.

In paleontology, Osborn's researches have been largely with the Reptilia and Mammalia, partly morphological, but also taxonomic and evolutional. Faunistic studies have also been made of the mammals. Of his published volumes the most important are, first, the Age of Mammals (1910), in which he treats not of evolutionary series of phylogenies, but of faunas and their origin, migrations, and extinctions, and of the correlation of Old and New World Tertiary deposits and their contents. Men of the Old Stone Age (1916) is an exhaustive treatise and is the first full and authoritative American presentation of what has been discovered up to the present time throughout the world in regard to human prehistory. In his latest volume, The Origin and Evolution of Life (1917), Osborn presents a new energy conception of evolution and heredity as against the prevailing matter and form conceptions. In this volume there is summed up the whole story of the origin and evolution of life on earth up to the appearance of man. This last book is novel in its conceptions, but it is too early as yet to judge

of the acceptance of Osborn's theses by his fellow workers in science.

Since the death of Professor Marsh, Osborn has served as vertebrate paleontologist to the United States Geological Survey, and has in charge the carrying through to completion of the many monographs proposed by his distinguished predecessor. One of these, that on the horned dinosaurs, has been completed by Hatcher and Lull (1907), another on the stegosaurian dinosaurs has been carried forward by C. W. Gilmore of the United States National Museum, while under Osborn's own hand are the memoirs on the titanotheres (aided by W. K. Gregory), the horses, and the sauropod dinosaurs. Of these, the first, when it shall have been completed, promises to be the most monumental and exhaustive study of a group of fossil organisms ever undertaken.

As a leader in science, a teacher and administrator, Professor Osborn's rank is high among the leading vertebratists. He is remarkably successful in his choice of assistants and in stimulating them in their productiveness so that their combined results form a very considerable share of the later literature in America.

The ninth decade ushered in the work of a valuable group of students, of whom John Bell Hatcher should be mentioned in particular, as his work is done. Graduate of Yale in 1884, he spent a number of years assisting his teacher, Professor Marsh, mainly in the field, collecting during that time, either for Yale or for the United States Geological Survey, an enormous amount of very fine material, especially from the West, although he also collected in the older Tertiary and Potomac beds near Washington. In the West he secured no fewer than 105 titanothere skulls, explored the Tertiary, Judith River, and Lance formations, collected and in fact virtually discovered the remains of the Cretaceous mammals and of the horned dinosaurs which he was later privileged to describe. He then (1893) went to Princeton, which he served for seven years, his principal work being explorations in Patagonia for the E. and M. Museum, one direct result of which was the publication of a large quarto on the narrative of the expedition and the geography and ethnography of the region. Going to the Carnegie

Museum in Pittsburgh in 1900, Hatcher carried forward the work of exploration and collecting begun for that institution by Wortman, and as a partial result prepared many papers, the principal ones being memoirs on the dinosaurs *Haplocanthosaurus* and *Diplodocus*. In 1903, with T. W. Stanton of the United States Geological Survey, Hatcher explored the Judith River beds and together they settled the vexatious problem of their age, the published results appearing in 1905, after Hatcher's death. His last piece of research, begun in 1902 and continued until his death in 1904, was an elaborate monograph on the Ceratopsia, one of the many projected by Marsh. Of this memoir Hatcher had completed some 150 printed quarto pages, giving a rare insight into the anatomy of these strange forms. The final chapters, however, which were based very largely upon Hatcher's own opinions, had to be prepared by another hand.

Despite his early death, therefore, Hatcher rendered a very signal service to American paleontology—in exploration, stratigraphy, morphology, and systematic revision—and his activity in planning new fields of research, such, for instance, as the exploration of the Antarctic continent, gave promise of further high attainment, when his hand was arrested by death.

Summary.

It is not surprising that American vertebrate paleontology has arisen to so high a plane, when one considers the material at its disposal. Having a vast and virgin field for exploration, a sufficient number of collectors, some of whom have devoted much of their lives to the work, and a refinement of technique that permitted the preservation of the fragmental and ill conserved as well as the finer specimens, the results could hardly have been otherwise. Thus it has been possible to secure material almost unique throughout the world for extent, for completeness, and for variety. To this must be added a certain American daring in the matter of the restoration of missing portions, both of the individual bones and of the skeleton as a whole, such as European conservatism will not as a rule permit. This work has for the most part been done after the most painstaking comparison and

research and is highly justified in the accuracy of the results, which render the fabric of the skeleton much more intelligible, both to the scientist and to the layman. Material once secured and prepared is then mounted, and here again American ingenuity has accomplished some remarkable results. Some of the specimens thus mounted are so small and delicate as to require holding devices comparable to those for the display of jewels; yet others—huge dinosaurs the bones of which are enormously heavy, but so brittle that they will not bear even the weight of a process unsupported—require a carefully designed and skilfully worked out series of supports of steel or iron which must be perfectly secure and at the same time as inconspicuous as possible. And of late the lifelike pose of the individual skeleton has been augmented by the preparation of groups of several animals which collectively exhibit sex, size, or other individual variations and the full mechanics of the skeleton under the varying poses assumed by the creature during life.

The work of further restoration has been rendered possible through comparative anatomical study, enabling us to essay restorations in entirety by means of models and drawings, clothing the bones with sinews and with flesh and the flesh with skin and hair, if such the creature bore; while the laws of faunal coloration have permitted the coloring of the restoration in a way which if not the actual hue of life is a very reasonable possibility.

Thus the American paleontologists have blazed a trail which has been followed to good effect by certain of their Old World colleagues.

With such means and methods and such material available, it is again not surprising that American paleontology has furnished more and more of the evidences of evolution, and disclosed to the eyes of scientists animal relationships which were undreamed of by the systematist whose research dealt only with the existing. It has also explained some vexatious problems of animal distribution and of extinction, and has connected up cause and effect in the great evolutionary movements which are recorded.

The results of systematic research have added hosts of new genera and species and of families, but of orders there are relatively few. Nevertheless a number,

especially among reptiles and mammals, have come to light as the fruits of American discovery. But aside from the dry cataloguing of such groups, the American systematists have worked out some very remarkable phylogenies and have thus clarified our vision of animal relationships in a way which the recent zoologist could never have done. In this connection, the Permian vertebrates, which have been collected and studied with amazing success, principally by Williston and Case, should be mentioned, although the work is yet incomplete. Some of these forms are amphibian, others reptilian, yet others of such character as to link the two classes as transitional forms. Of the Mesozoic reptiles, a very remarkable assemblage has come to light, in a degree of perfection unknown elsewhere. These are dinosaurs, of which several phyla are now known: carnivores both great and small, some of the latter being actually toothless; Sauropoda, whose perfection and dimensions are incomparable except for those found in East Africa; and predentates, armored, unarmored, and horned, the last exclusively American. The unarmored trachodonts are now known in their entirety, for not only has our West produced articulated skeletons but mummified carcasses whose skin and other portions of their soft anatomy are represented, and which are thus far without a parallel elsewhere in the world. Other reptilian groups are well known, notably the Triassic ichthyosaurs, and the mosasaurs and plesiosaurs of the Kansas chalk. The last formation has also produced toothed birds, *Hesperornis* and *Ichthyornis,* which again are absolutely unique.

But it is in the mammalian class that the phylogenies become so highly complete and of such great importance as evolutionary evidences, for nowhere else than in our own West have such series been found as the Dinocerata and creodonts among archaic forms, the primitive primates from the Eocene, the carnivores such as the dogs and cats and mustellids, but especially the hoofed orders such as the horses. Of these hoofed orders, the classic American series of horses is complete, that of the camels probably no less so, while much is known of the deer and oreodonts, the last showing several parallel

phyla, and of the proboscideans, which while having their pristine home in the Old World nevertheless soon sought the new where their remains are found from the Miocene until their final and apparently very recent extinction. These creatures show increase of bulk, perfection of feet and teeth, development of various weapons, horns and antlers, which may be studied in their relationship with the other organs to make the evolving whole, or their evolution may be traced as individual structures which have their rise, culmination, and sometimes their senile atrophy in a way comparable to that of the representatives of the order as a whole. Thus, for example, Osborn has traced the evolution of the molar teeth, and Cope of the feet, while Marsh has shown that brain development runs a similar course and that its degree of perfection within a group is a potent factor for survival.

As a student of evolution, the paleontologist sees things in a very different light from the zoologist. The latter is concerned largely with matters of detail—with the inheritance of color or of the minor and more superficial characteristics of animals—and the period of observation of such phenomena is of necessity brief because of the mortality of the observer. Whereas the paleontologist has a perspective which the other lacks, since for him time means little in the terms of his own life, and he can look into the past and see the great and fundamental changes which evolution has wrought, the rise of phyla, of classes, of orders, and he alone can see the orderliness of the process and sense the majesty of the laws which govern it.

Influence of the American Journal of Science.

The influence of the American Journal of Science as a medium for the dissemination of the results of vertebrate research has been in evidence throughout this discussion, but it were well, perhaps, to emphasize that service more fully. The Journal was, as we have seen, the chief outlet for Professor Marsh's research, for there were published in it during his lifetime no fewer than 175 papers descriptive of the forms which he studied, as well as a great part of the material in the published monographs. As Marsh

left very few manuscript notes, the importance of these frequent publications in thus setting forth much that he thought and learned concerning the material is very great indeed. The combined titles of all other authors in the Journal in this line of research for the century of its life fall far short of the number produced by Marsh alone, as they include 136 all told, but the range of subjects is highly representative of the entire field of vertebrate research. It should be borne in mind, moreover, that Leidy, Cope, and Osborn each had another medium of publication, which of course is true of other workers in the great museums such as the American, National, and Carnegie, all of which issue bulletins and quarto publications for the purpose of disseminating the work of their staff. Many of the earlier announcements of the discovery of vertebrate relics appeared in the Journal, as did practically all the literature of the science of fossil footprints (ichnology), except of course the larger quartos of Hitchcock and Deane. Of the footprint papers by Hitchcock, Deane, and others, there were no fewer than thirty-two, with a number of additional communications on attendant phenomena bones and plants.

Up to 1847, except for a few foreign announcements, the Journal published almost exclusively on eastern American paleontology, the only exception being a notice of bones from Oregon by Perkins in 1842. In 1847 came the announcement of a western "Palæothere" by Prout, which marked the beginning of the researches of Leidy and others in the Bad Lands of the great Nebraska plains. The Journal thenceforth published paper after paper on forms from all over North America, and on all aspects of our science: discovery, systematic description, faunal relationships, evolutionary evidences—thus showing that breadth and catholicity which has made it so great a power in the advancement of science.

VII

THE RISE OF PETROLOGY AS A SCIENCE

By LOUIS V. PIRSSON

THIS chapter is intended to present a brief sketch of the progress of the science of petrology from its early beginnings down to the present time. The field to be covered is so large that this can be done only in broadest outline, and it has therefore been restricted chiefly to what has been accomplished in America. Although the period covered by the life of the Journal extends backward for a century it is, however, practically only within the last fifty years that the rocks of the earth's crust have been made the subject of such systematic investigation by minute and delicately accurate methods of research as to give rise to a distinct branch of geologic science. It is not intended of course to affirm by this statement that the broader features of the rocks, especially those which may be observed in the field and which concern their relations as geologic masses, had not been made the object of inquiry before this time, since this is the very foundation of geology itself. Moreover, a certain amount of investigation of rocks, as to the minerals of which they were composed, the significance of their textures, and their chemical composition, had been carried out, concomitant with the growth from early times of geology and mineralogy. Thus, in 1815, Cordier by a process of washing separated the components of a basalt and by chemical tests determined the constituent minerals. At the time the Journal was founded, and for many years following, the genesis of rocks, especially of igneous rocks, was a subject of inquiry and of prolonged discussion. The aid of the rapidly growing science of chemistry was invoked by the geologists and analyses of rocks were made in the attempt to throw light on impor-

tant questions. It is remarkable, also, how keen were the observations that the geologists of those days made upon the rocks, as to their component minerals and structures, aided only by the pocket lens. Many ideas were put forward, the essentials of which have persisted to the present day and have become interwoven into the science, whereas others gave rise to contentions which have not yet been settled to the satisfaction of all. At times in these earlier days the microscope was called into use to help in solving questions regarding the finer grained rocks, but this employment, as Zirkel has shown, was merely incidental, and no definite technique or purpose for the instrument was established.

On the other hand, the fact that up to the middle of the last century a large store of information relating to the occurrence of rocks, and to the mineral composition of those of coarser grain, and somewhat in respect to their structure, had been accumulated, caused attempts in one way or another to find means of coördinating these data and to produce classifications, such as those of Von Cotta and Cordier. The history of these attempts at classification, before the revelations made by the use of the microscope had become general, has been admirably reviewed by Whitman Cross[1] and need not be further enlarged upon here.

That a considerable amount of work was done along chemical lines also is testified to by the publication of Roth's Tabellen in 1861, in which all published analyses of rocks up to that date were collected. What was accomplished during this period was done chiefly on the continent of Europe, and little attention had been paid to the subject of rocks either in America or in Great Britain—even so late as 1870 Geikie remarks, as referred to by Cross,[2] that there was no good English treatise on petrography, or the classification and description of rocks. In this country still less had been accomplished, interest being almost wholly confined to the vigorous and growing sciences of geology and mineralogy. This was natural, for mineralogy is the chief buttress on which the structure of petrology rests and must naturally develop first, especially in a relatively new and unexplored region, whose mineral resources first attract attention.

The geologists in carrying out their studies also observed the rocks as they saw them in the field and made incidental reference to them, but investigations of the rocks themselves was very little attempted. An inspection of the first two series of the Journal shows relatively little of importance in petrology published in this country; a few analyses of rocks, occasional mention of mineral composition, of weathering properties, and notices of methods of classification proposed by French and German geologists nearly exhaust the list.

Introduction of the Microscope.

The beginnings of a particular branch of science are generally obscure and rooted so imperceptibly in the foundations on which it rests that it is difficult to point to any particular place in its development and say that this is the start. There are exceptions of course, like the remarkable work of Willard Gibbs in physical chemistry, and it may chance that the happy inspiration of a single worker may give such direction to methods of investigation as to open the gates into a whole new realm of research, and to thus create a separate scientific field, as happened in Radiochemistry.

This is what occurred in petrology when Sorby in England, in 1858,[3] pointed out the value of the microscope as an instrument of research in geologic investigations, and demonstrated that its employment in the study of thin sections of rocks would yield information of the highest value. Others beside Sorby had made use of the microscope, as pointed out by Zirkel,[4] but, as he indicates, no one before him had recognized its value. During the next ten years or so, however, its recognition was very slow and the papers published by Sorby himself were mainly concerned in settling very special matters.

As Williams[5] has suggested, the greatest service of Sorby was, perhaps, his instructing Zirkel in his ideas and methods, for the latter threw himself whole-heartedly into the study of rocks by the aid of the microscope and his discoveries stimulated other workers in this field in Germany, his native country, until the dawning science of petrology began to assume form. A further step for-

ward was taken in 1873 in the appearance of the text-books of Zirkel[6] and Rosenbusch[7] which collated the knowledge which had been gained and furnished the investigator more precise methods of work. It is difficult for the student of to-day to realize how much had been learned in the interval and, for that matter, how much has been gained since 1873, without an inspection of these now obsolete texts. In 1863, Zirkel, who was then at the beginning of his work, said in his first paper presented to the Vienna Academy of Sciences[8] that if he confined himself chiefly to the structure of the rocks investigated and of their component minerals, and stated little as to what these minerals were, the reason for that was because "although the microscope serves splendidly for the investigation of the former relations, it promises very little help for the latter. Labradorite, oligoclase and orthoclase, augite and hornblende, minerals whose recognition offers the most important problems in petrography, in most cases cannot be distinguished from one another under the microscope." How little could Zirkel have foreseen, at this time, less than forty years later, that not only could labradorite be accurately determined in a rock-section, but that in a few minutes by the making of two or three measurements on a properly selected section, its chemical composition and the crystallographic orientation of the section itself could be determined!

The Thin Section.

Before going further we may pause here a moment to consider the origin and development of the thin section, without which no progress could have been made in this field of research. When we reflect upon the matter, it seems a marvelous thing indeed that the densest, blackest rock can be made to yield a section of the 1/1000 of an inch in thickness, so thin and transparent that fine printing can be easily read through it, and transmitting light so clearly that the most high-powered objectives of the microscope can be used to discern and study the minutest structures it presents with the same capacity that they can be employed upon sections of organic material prepared by the microtome. This is no small achievement.

The first thin sections appear to have been prepared in 1828 by William Nicol of Edinburgh, to whom we owe the prism which carries his name. He undertook the making of sections from fossil wood for the purpose of studying its structure. The method he developed was in principle the same as that employed to-day, where machinery is not used; that is, he ground a flat smooth surface upon one side of a chip of his petrified wood, then cemented this to a bit of glass plate with Canada balsam, and ground down the other side until the section was sufficiently thin. This method was used by others for the study of fossil woods, coal, etc., but it was not applied to rocks until 1850, when Sorby used it for investigating a calcareous grit. Oschatz, in Germany, also about this time independently discovered the same method. A further advance was made in melting the cement, floating off the slice, and transferring it to a suitable object-glass with cover, a process still employed by many; though most operators now cement the first prepared surface of the rock chip directly to the object-glass, and mount the section without transferring it.

Next came the use of machinery to save labor in grinding, and another step was made in the introduction of the saw, a circular disk of sheet iron whose edge was furnished with embedded diamond dust. This makes it possible to cut relatively thin slices with comparative rapidity, but the final grinding which requires experience and skill must still be done by hand. Carborundum has also largely replaced emery. The skill and technique of preparers has reached a point where sections of rocks of the desired thinness (0·001 inch), and four or five inches square have been exhibited.

The Era of Petrography.

In these earlier days of the science, as noted above, great difficulty was at first experienced in the recognition of the minerals as they were encountered in the study of rocks under the microscope. At that time the chemical composition and outward crystal form of minerals were relatively much better known than their physical and, especially, their optical properties and constants. Some

beginnings in this had been made by Brewster, Nicol, and other physicists, and the mineralogists had commenced to study minerals from this viewpoint. Especially Des Cloiseaux had devoted himself to determining the optical properties of many minerals, and the writer, when a student in the laboratory of Rosenbusch in 1890, well recalls the tribute that he paid to the work of Des Cloiseaux for the aid which it had afforded him in his earlier researches in petrography.

The twenty years following the publication of the texts of Rosenbusch and Zirkel may be characterized as the era of microscopical petrography. A distinction is drawn here between the latter word and petrology, a distinction often overlooked, for *petrography* means literally the description of rocks, whereas *petrology* denotes the science of rocks. As time passed the broader and more fundamental features of rocks, especially of igneous and metamorphic rocks, in addition to their mineral constitution, were more studied and gained greater recognition, petrography gradually became a department of the larger field of petrology—the science of to-day.

The use of the microscope, as soon as the method became more generally understood, opened up so vast a field for investigation that at first the study and description of the rocks seemed of prime importance. This was natural, for hitherto the finer grained rocks had for the most part defied any adequate elucidation and here was a key which enabled one to read the cipher. A flood of literature upon the composition, structure, and other characters of rocks from all parts of the world began to appear in ever increasing volume. The demands of the petrographers for a greater and more accurate knowledge of the physical and optical constants of minerals stimulated this side of mineralogy, and increasing attention was given to investigations in this direction. No definite line between the two closely related sciences could be drawn, and a large part of the work published under the heading of petrography could perhaps be as well, or better, described under the title of micro-mineralogy. To some, in truth, the rocks presented themselves simply as aggregates of minerals, occurring in fine grains.

The work of the German petrographers attracted

attention and drew students from all parts of the world to their laboratories, especially to those of Zirkel and Rosenbusch. The great opportunities, facilities, and freedom for work which the German universities had long offered to foreign students of science naturally encouraged this. In France a brilliant school of petrologists, under the able leadership of Michel-Lévy and Fouqué, had arisen whose work has been continued by Barrois, Lacroix and others, but the rigid structure of the French universities at that period did not permit of the offering of great inducements for the attendance of foreign students. The work of the French petrographers will be noticed in another connection.

In Great Britain, the home of Sorby, the new science progressed at first slowly, until it was taken up by Allport, Bonney, Judd, Rutley, and others. In 1885 the evidence of the advance that had been made and of the firm basis on which the new science was now placed appeared in Teall's great work, "British Petrography," which marked an epoch in that country in petrographic publication. This work was of importance also in another direction than that of descriptive petrography, in that it contains valuable suggestions for the application of the principles of modern physical chemistry in solving the problems of the origin of igneous rocks. In it, as in the publications of Lagorio, we see the passage of the petrographic into the petrologic phase of the science.

The earliest publication in America of the results of microscopic investigation of rocks that the writer has been able to find is by A. A. Julien and C. E. Wright, chiefly on greenstones and chloritic schists from the iron-bearing regions of upper Michigan.[9] Naturally, it was of a brief and elementary character. In 1874 E. S. Dana read a paper before the American Association for the Advancement of Science on the result of his studies on the "Trap-rocks of the Connecticut valley," an abstract of which was published in this Journal.[10] Meanwhile Clarence King, in charge of the 40th Parallel survey, feeling the need of a systematic study of the crystalline rocks which had been encountered, and finding no one in this country prepared to undertake it, had induced Zirkel to give his attention to this task. The

result of this labor appeared in 1876 in a fine volume[11] which attracted great attention. In the same year appeared also petrographical papers by J. H. Caswell,[12] E. S. Dana[13] and G. W. Hawes.[14] The latter devoted himself almost entirely to this field of research and may thus, perhaps, be termed the earliest of the petrographers in this country. His work, "The Mineralogy and Lithology of New Hampshire," issued in 1878 as one of the reports of the State Survey under Prof. C. H. Hitchcock, was the first considerable memoir by an American. This was followed by various papers, one on the "Albany Granite and its contact phenomena,"[15] being of especial interest as one of the earliest studies of a contact zone, and in the fullness of methods employed in attacking the problem forecasting the change to the petrology era.

During the ten years following, or from 1880 to 1890, the new science of petrography flourished and grew exceedingly. Many young geologists abroad devoted themselves to this field of research and the store of accumulated knowledge concerning rocks from all parts of the world, and their relations grew apace. The work of Teall has been noticed and among others might be mentioned the name of Brögger, whose first contribution[16] in this field gave evidence that his publications would become classics in the science.

In America there appeared in this period a number of eager workers, trained in part in the laboratories of Rosenbusch and Zirkel, whose researches were destined to place the science on the secure footing in this country which it occupies to-day. Among the earlier of these may be mentioned Whitman Cross, R. D. Irving, J. P. Iddings, G. H. Williams, J. F. Kemp, J. S. Diller, B. K. Emerson, M. E. Wadsworth, G. P. Merrill, N. H. Winchell, and F. D. Adams in Canada. Others were added yearly to this group. As a result of their work a constantly growing volume of information about the rocks of America became available, and one has only to examine the files of the Journal and other periodicals and the listed publications of the National and State Surveys to appreciate this.

In the Journal, for example, we may refer to papers[17]

by Emerson on the Deerfield dike and its minerals, and on the occurrence of nephelite syenite at Beemersville, N. J.; to various interesting articles by Cross on lavas from Colorado and the pneumatolytic and other minerals associated with them; to important papers by Iddings on the rocks of the volcanoes of the Northwest, and those of the Great Basin, to primary quartz in basalt, and the origin of lithophysæ; to the results of researches by G. H. Williams on the rocks of the Cortlandt series, and on peridotite near Syracuse, N. Y.; to papers by Diller on the peridotites of Kentucky, and recent volcanic eruptions in California; to articles by R. D. Irving on the copper-bearing and other rocks of the Lake Superior region, and to Kemp on dikes and other eruptives in southern New York and northern New Jersey. Other publications would greatly extend this list.

The Petrologic Era.

As the chief facts regarding rocks, especially igneous rocks, as to their mineral and chemical composition, their structure and texture and the limits within which these are enclosed, became better known; and the relations, which these bear to the associations of rocks and their modes of occurrence, began to be perceived, the science assumed a broader aspect. The perception that rocks were no longer to be regarded merely as interesting assemblages of minerals, but as entities whose characters and associations had a meaning, increased. More and better rock analyses stimulated interest on the chemical side and this and the genesis of their minerals led to a consideration of the magmas and their functions in rock-making. The fact that the different kinds of rocks were not scattered indiscriminately, but that different regions exhibited certain groupings with common characters, was noticed. These features led to attempts to classify igneous rocks on different lines from those hitherto employed, and to account for their origin on broad principles. In other words, the descriptive science of petrography merged into the broader one of petrology. No exact time can be set which marks this passage, since the evolution was gradual. Yet for this

country, in reviewing the literature, for which the successive issues of the "Bibliography of North American Geology" published by the U. S. Geological Survey has been of the greatest value; the writer has been struck by the fact that in the first volume containing the index of papers down to and including 1891, the articles on subjects of this nature are listed under the heading of *petrography*, whereas in the second volume (1892-1900) they are grouped under *petrology* and the former heading is omitted. A justification for this is found in examining the list of publications and noting their character. With some reason, therefore, the beginning of this period may be placed as in the early years of this decade. Furthermore, it was at this time that the great work of Zirkel[18] began to appear, which sums up so completely the results of the petrographic era. Rosenbusch[19] was formulating more definitely his views on the division of rocks into magmatic groups, as displayed by their associations in the field, and using this in classification; an idea which, appearing first in the second edition of his "Physiographie der massigen Gesteine," finds fuller development in the third and last editions of this work. In this country Iddings[20] published an important paper, in which the family relationships of igneous rocks and the derivation of diverse groups from a common magma by differentiation are clearly brought out. The fundamental problems underlying the genesis of igneous rocks had now been clearly recognized, and with this recognition the science passed into the petrologic phase. Brögger[21] also had ascribed to the alkalic rocks of South Norway a common parentage and had pointed out their regional peculiarities.

From this time forward an attempt may be noted to find an analogy between rocks and the forms of organic life and to apply those principles of evolution and descent, which have proved so fruitful in the advancement of the biological sciences, to the genesis and classification of igneous rocks. This, perhaps, has on the whole been more apparent than real, in the constant borrowing of terms from those sciences to express certain features and relationships observed, or imagined, to obtain among rocks. Nevertheless, the perception of certain relations

which we owe so largely to Rosenbusch and to Brögger[22] has proved of undoubted value in furnishing a stimulus for the investigation of new regions, and in affording indications of what the petrologist should anticipate in his work.

Thus, the labors of the men previously mentioned, with those of Bayley, Bascom, Cushing, Daly, Lane, Lawson, Lindgren, Pirsson, J. F. Williams, Washington, and others, have thrown a flood of light upon the igneous rocks of this continent, and has made it possible to draw many broad generalizations concerning their origin and distribution. Thus, the differentiated laccoliths of Montana[23] have been of service in affording clear examples of the process of local differentiation. Many papers published in the Journal during the last twenty years show this evolution and growth of petrological ideas. The contributions from American sources during this later period, and of which those in the Journal form a considerable fraction, have indeed been of great weight in shaping the development and future of the science.

By referring to the files of the Journal, it will be seen that they cover a continually widening range of subjects concerning rocks, and articles of theoretical interest are more and more in evidence, along with those of a purely descriptive character.[24] Thus we find discussions by Becker on the physical constants of rocks, on fractional crystallization, and on differentiation; by Cross on classification; by Adams on the physical properties of rocks; by Daly on the methods of igneous intrusion; by Wright on schistosity; by Fenner on the crystallization of basaltic magma; by Bowen on differentiation by crystallization; by the writer on complementary rocks and on the origin of phenocrysts; by Smyth on the origin of alkalic rocks; by Murgoci on the genesis of riebeckite rocks; and by Barrell on contact-metamorphism. These may serve as examples, selected almost at random, from the files of the Journal, and we find with them articles descriptive of the petrology of many particular regions, which often contain also matter of general interest and importance, such as papers by Lindgren on the granodiorite and related rocks of the Sierra Nevada; by Ransome on latite; by Cross on the Leucite Hills; by

Hague on the lavas of the Yellowstone Park; by Pogue on ancient volcanic rocks from North Carolina; by Warren on peridotites from Cumberland, R. I.; on sandstone from Texas by Goldman; and on the petrology of various localities in central New Hampshire by Washington and the writer. Such a list could of course be much extended and other papers of importance be cited, but enough has been said to indicate how important a repository of the results of petrologic research the Journal has been and continues to be.

In thus looking backward over the list of active workers we are involuntarily led to pause and reflect how great a loss American petrology has sustained in the premature death of some of its most brilliant and promising exponents; it is only necessary to recall the names of R. D. Irving, G. H. Williams, G. W. Hawes, J. F. Williams and Carville Lewis, to appreciate this.

The store of material gathered during these years has led to the publication of extensive memoirs, in which the science is treated not from the older descriptive side, but from the theoretical standpoint and of classification.[25] In these works strong divergencies of views and opinions are observed, which is a healthy sign in a developing science.

It should be also noted that along with this evolution on the theoretical side there has been a constant improvement in the technique of investigating rocks. It is only necessary to compare the older handbooks of Zirkel and Rosenbusch with the many modern treatises on petrographic methods to be assured of this.[26] It is due on the one hand to the vast amount of careful work which has been done in accurately determining the physical constants of rock-minerals* and in arranging these for their determination microscopically, as in the remarkable studies on the feldspars by Michel-Lévy, and on the other in researches on the apparatus employed, and in conse-

* We may mention here, for example, the work in mineralogy of Penfield, noticed in the accompanying chapter on mineralogy. In addition to the accurate determination of the composition and constants of many minerals, some of which have importance from the petrographic standpoint, we owe to him more than anyone the recognition of fluorine and hydroxyl in a variety of species, and thereby the perception of their pneumatolytic origin. His papers have been published almost entirely in the Journal.

quent improvements in them and in ways of using them, as exemplified in the delicately accurate methods introduced by Wright.[27] The development of the microscope itself as an instrument of research in this field and in mineralogy deserves a further word in this connection. The first step toward making the ordinary microscope of special use in this way was taken by Henry Fox Talbot of England, when he introduced in 1834 the employment of the recently invented nicol prisms for testing objects in polarized light. The modern instrument may be said to date from the design offered by Rosenbusch in 1876. Since that time there have been constant improvements, almost year by year, until the instrument has become one of great precision and convenience, remarkably well adapted for the work it is called upon to perform, with special designs for various kinds of use, and an almost endless number of accessory appliances for research in different branches of mineralogy and crystallography, as well as in petrography proper.[28] This also calls to mind the fact that for the convenience of those who are not able to use the microscope special manuals of petrology have been prepared in which rocks are treated from the megascopic standpoint.[29]

Metamorphic Rocks.

In this connection the metamorphic rocks should not be forgotten. They afford indeed the most difficult problems with which the geologist has to deal; every branch of geological science may in turn be called upon to furnish its quota for help in solving them. Under the attack of careful, accurate and persistent work in the field, under the microscope and in the chemical laboratory, with the aid of the garnered knowledge in petrology, stratigraphy, physiography, and other fields of geologic science, their mystery has in large part given way. The inaugural work of Lehmann, Lossen, Barrois, Bonney, Teall, and other European geologists, was paralleled in America by that of R. D. Irving, owing to whose efforts the Lake Superior region became the chief place of study of the metamorphic rocks in this country. Irving soon obtained the assistance of G. H. Williams, who had been engaged in the study of such rocks, and the

latter published a memoir on the greenstone schist areas of Menominee and Marquette in Michigan[30] which will always remain one of the classics in the literature of metamorphic rocks. Irving's own contributions to petrology, though valuable, were cut short by his untimely death, but the study of this region under the direction of his associate and successor, C. R. Van Hise, with his co-laborers, has yielded a mass of information of fundamental importance in our understanding of metamorphism and the crystalline schists. Its fruitage appears in the memoir by Van Hise[31] which is the authoritative work of reference on metamorphism, and in various publications by him and his assistants, Bayley, Clements, Leith, and others. The work of the Canadian geologists, and of Kemp, Cushing, Smyth and Miller in the Adirondack region, should also be mentioned in connection with this field of petrology.

Chemical Analyses of Rocks.

It has been previously pointed out that, as the science of petrology grew, chemical investigations of rocks in bulk were undertaken. The object of such analyses was to obtain on the one hand a better control over the mineral composition and on the other to gain an idea of the nature of the magmas from which igneous rocks had formed. The earliest analysis of an American rock of which I can find record is of a "wacke" by J. W. Webster given in the first volume of the Journal, page 296, 1818.

During the next 40 years a few occasional analyses were undertaken by American chemists, by C. T. Jackson, T. Sterry Hunt, and others. In 1861, Justus Roth published the first edition of his Tabellen, in which he included all analyses which had been made to that date and which he considered were worthy of preservation. Although, naturally, from the status of analytical chemistry up to that time, most of these would now be considered rather crude, the publication of the work was of great service and marked an epoch in geochemistry. In these tables Roth lists four analyses of American igneous rocks, two from the Lake Superior region by Jackson and J. D. Whitney and two by European chemists, one of whom was Bunsen. The material of the last two was a

"dolerite" and the same locality is given for each—"Sierra Nevada between 38° and 41°" which was probably considered quite precise for western America in those days.

From these feeble beginnings the forward progress of petrology on the chemical side in this country has been a steady one until its development has reached the point which will be indicated in what follows.

The collection of material by the various State surveys and by those initiated by the National Government led to an increasing number of rocks being analyzed during the petrographic period. These became also increasingly good in quality, like those published by G. W. Hawes in his papers. When, however, chemists were appointed to definite positions on the staffs of the Government surveys and especially when, after the organization of the U. S. Geological Survey in 1879, a general central laboratory was founded in 1883 with F. W. Clarke in charge, then a new era in the chemical investigation of rocks may be said to have started. In this connection should be mentioned the work of W. F. Hillebrand, who set a standard of accuracy and detail in rock analysis which had not hitherto been attempted. As a consequence of his accurate and thorough methods and results the mass of analyses performed by him and his fellow chemists in this laboratory affords us the greatest single contribution to chemical petrology which has been made. Up to January, 1914, the report of Clarke[32] lists some 8000 analyses of various kinds made in this laboratory for geologic purposes. Nearly everywhere also a great improvement in the quality of rock-analyses is to be noted, and in the manuals of Hillebrand[33] and Washington[34] the rock analyst has now at his command the methods of a greatly perfected technique which should insure him the best results.

Roth's Tabellen have been previously mentioned; several supplements were published, but after his death a long interval elapsed before this convenient and useful work was again taken up by Washington[35] and Osann.[36] A new edition of Washington's Tables has recently been published, listing some 8600 analyses of igneous rocks made up to the close of 1913.[37]

On the theoretical side also, where petrology passes into geology, the investigator of to-day will find a mass of most useful and accurate data well discussed in the modern representative of Bischof's Chemical Geology—Clarke's Data of Geochemistry.[38] The advance on the chemical side, therefore, has been quite commensurate with that in the microscope as an instrument, and in the results obtained by it.

Physico-Chemical Work.

The study of geological results by experimental methods, which should gain information concerning the processes by which those results are caused, and the conditions under which they operate, has been from the earliest days of the developing science recognized as most important, and the record of the literature shows considerable was done in this direction. Experimental work in modern petrology may, however, be considered to date from 1882 when Fouqué and Michel-Lévy[39] published the results of their extensive researches on the synthesis of minerals and rocks by pyrogenous methods. The brilliant experiments of the French petrologists at once attracted attention, and since that time a considerable volume of valuable work has been done in this field by a number of men, among whom may be mentioned Morozewicz,[40] Doelter,[41] Tamman,[42] and Meunier.[43] As this work continued the results of the rapid advances made in physical chemistry began to be applied in this field with increasing value. To J. H. L. Vogt we owe a valuable series of papers,[44] in which the formation of minerals and rocks from magmas is treated from this standpoint. Most important of all for the future of petrology has been the founding in Washington of the splendid research institution, the Carnegie Geophysical Laboratory, under the leadership of Dr. A. L. Day with its corps of trained physicists, chemists and petrologists, devoted to the solving of the problems which the progress of geological science raises. The publications of this institution (many of them published in the Journal) are too numerous to be mentioned here; many of them treat successfully of matters of the greatest importance in petrology. This is an earnest of what we may hope in

the future. The accumulation of the exact physical and chemical data, which is its aim, will serve as a necessary check to hypothetical speculation and bring petrology, and especially petrogenesis, in line with the other more exact sciences by furnishing quantitative foundations for its structure of theory to rest upon.

While the achievements of this great organization seem to minimize the work of the individual investigator in this field, he may take heart by observing the important results on the strength of rocks under various conditions which have been obtained by Adams in recent years, data of wide application in theoretical geology. In this field also a special text has appeared in which the principles and acquired data are given.[45]

Summary.

In this brief retrospect, giving only the barest outlines and omitting from necessity much of importance, we have seen petrology grow from occasional crude experiments into a fully organized science in the last half century. It has to-day a well-perfected technique, a large volume of literature, texts treating of general principles, of methods of work, descriptive handbooks on the morphological side, and has attained general recognition as a field, which, though not large, is worthy of the concentration of intellectual endeavor. Like other healthy growing organisms it has given rise to offshoots, and the sciences of metallography and of the micro-study of ore deposits, which are rapidly assuming form, have branched from it.

What of the future? The old days of mostly descriptive work, and of theorizing purely from observed results, have passed. The science has entered upon the stage where work and theory must be continually brought into agreement with chemical, physical and mathematical laws and data, and in the application of these new problems present themselves. As we climb, in fact, new horizons open to our view indicating fresh regions for exploration, for acquiring human knowledge and for our satisfaction.

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VIII

THE GROWTH OF MINERALOGY FROM 1818 TO 1918

By WILLIAM E. FORD

MINERALOGY to-day would certainly be generally considered one of the minor members of the group of the Geological Sciences. We commonly look upon it in the light of an useful handmaiden, whose chief function is to serve the other branches, and we are inclined to forget that, in reality, mineralogy was the first to be recognized and, with considerable truth, might be claimed as the mother of all the others. Minerals, because of their frequent beauty of color and form, and their uses as gems and as ornamental stones, were the first inorganic objects to excite wonder and comment and we find many of them named and described in very early writings. Theophrastus (368-284 B. C.), a famous pupil of Aristotle, wrote a treatise "On Stones" in which he collected a large amount of information about minerals and fossils. The elder Pliny (23-79 A. D.), more than three centuries later, in his Natural History, described and named many of the commoner minerals. At this time it was natural that no clear distinction should be drawn between minerals and rocks, or even between minerals and fossils. As long as all study of the materials of the earth's crust was concerned with their superficial characters, it was logical to include everything under the single head. There were some writers in the early centuries of the Christian era, however, who believed that fossils had been derived from living animals but the majority considered them to be only strange and unusual forms of minerals. During many succeeding centuries little was added to the general store of geological knowledge and it was not until the beginning of the sixteenth

century, that any further notable progress was made. Agricola (1494-1555) was a physician, who, for a time, lived in the mining district of Joachimstal. He studied and described the minerals that he collected there. He was the first to give careful and critical descriptions of minerals, of their crystals and general physical properties. Unfortunately, he also did not realize the fundamental distinction between fossils and minerals, and probably because of his influence this error persisted, even until the middle of the eighteenth century. But, naturally, as the number of scientific students increased, the number of those who rejected this conclusion grew, until at last, the true character of fossils was established. The keen interest in minerals and fossils which was aroused by this controversy, together with the rapid extension of mining operations, drew the attention of scientific men to other features of the earth's surface and led to a more extended investigation of its characters and thus to the development of geology proper. It is interesting to note also that mineralogy was the first of the Geological Sciences to be officially recognized and taught by the universities.

Although, as has been shown, the beginnings of mineralogy lie in the remote past, the science, as we know it to-day, can be said to have had practically its whole growth during the last one hundred years. Of the more than one thousand mineral species that may now be considered as definitely established hardly more than two hundred were known in the year 1800 and these were only partially described or understood. It is true that Haüy, the "father of crystallography," had before this date discovered and formulated the laws of crystal symmetry, and had shown that rational relations existed between the intercepts upon the axes of the different faces of a crystal. It was not until 1809, however, that Wollaston described the first form of a reflecting goniometer, and thus made possible the beginning of exact investigation of crystals. The distinctions between the different crystal groups were developed by Bernhardi, Weiss and Mohs between the years 1807 and 1820, while the Naumann system of crystal symbols was not proposed until 1826. The fact that doubly refracting minerals also polarize

light was discovered by Malus in 1808, and in 1813 Brewster first recognized the optical differences between uniaxial and biaxial minerals. The modern science of chemistry was also just beginning to develop at this period, enabling mineralogists to make analyses more and more accurately and thus by chemical means to establish the true character of minerals, and to properly classify them.

Franz von Kobell, on page 372 of his "Geschichte der Mineralogie," somewhat poetically describes the condition of the science at this period as follows: "With the end of the eighteenth and the commencement of the nineteenth centuries exact investigations in mineralogy first began. The mineralogist was no longer content with approximate descriptions of minerals, but strove rather to separate the essential facts from those that were accidental, to discover definite laws, and to learn the relations between the physical and chemical characters of a mineral. The use of mathematics gave a new aspect to crystallography, and the development of the optical relationships opened a magnificent field of wonderful phenomena which can be described as a garden gay with flowers of light, charming in themselves and interesting in their relations to the forces which guide and govern the regular structure of matter."

In the Medical Repository (vol. 2, p. 114, New York, 1799), there occurs the following notice: "Since the publication of the last number of the Repository an Association has been formed in the city of New York 'for the investigation of the Mineral and Fossil bodies which compose the fabric of the Globe; and, more especially, for the Natural and Chemical History of the Minerals and Fossils of the United States,' by the name and style of The American Mineralogical Society." With this announcement is given an advertisement in which the society "earnestly solicits the citizens of the United States to communicate to them, on all mineralogical subjects, but especially on the following: 1, concerning stones suitable for gun flints; 2, concerning native brimstone or sulphur; 3, concerning salt-petre; 4, concerning mines and ores of lead." Further the society asks "that

specimens of all kinds be sent to it for examination and determination."

This marks apparently the beginning of the serious study of the science of mineralogy in the United States. From this time on, articles on mineralogical topics appeared with increasing frequency in the Medical Repository. Most of these were brief and were largely concerned with the description of the general characters and modes of occurrence of various minerals. Nothing of much moment from the scientific point of view appeared until many years later, but the growing interest in things mineralogical was clearly manifest. An important stimulus to this increasing knowledge and discussion was furnished by Col. George Gibbs who, about the year 1808, brought to this country a large and notable mineral collection. In the Medical Repository (vol. 11, p. 213, 1808), is found a notice of this collection, a portion of which is reproduced below:

"Gibbs' grand Collection of Minerals.

One of the most zealous cultivators of mineralogy in the United States is Col. G. Gibbs of Rhode Island and his taste and his fortune have concurred in making him the proprietor of the most extensive and valuable assortment of minerals that probably exists in America.

This rich collection consists of the cabinets possessed by the late Mons. Gigot D'Orcy of Paris and the Count Gregoire de Rozamonsky, a Russian nobleman, long resident in Switzerland. To which the present proprietor has added a number, either gathered by himself on the spot, or purchased in different parts of Europe . . . The whole consists of about twenty thousand specimens. A small part of this collection was opened to amateurs at Rhode Island, the last summer, and the next, if circumstances permit, the remainder will be exposed."

In 1802 Benjamin Silliman was appointed professor of chemistry and mineralogy in Yale College. After the Gibbs Collection was brought to America he spent much time with the owner in studying it and, as a result, Col. Gibbs offered to place the collection on exhibition in New Haven if suitable quarters would be furnished by the college. This was quickly accomplished and in 1810, 1811 and 1812 the collection was transferred to New Haven

and arranged for exhibition by Col. Gibbs. Later, in 1825, it was purchased by Yale and served as the nucleus about which the present Museum collection of the University has been formed. There is no doubt but that the presence at this early date of this large and unusual mineral collection had a great influence upon the development of mineralogical science at Yale, and in the country at large.

In the year 1810 Dr. Archibald Bruce started the "American Mineralogical Journal," the title page of which reads in part as follows: "The American Mineralogical Journal, being a Collection of Facts and Observations tending to elucidate the Mineralogy and Geology of the United States of America, together with other Information relating to Mineralogy, Geology and Chemistry, derived from Scientific Sources." Unfortunately the health of Dr. Bruce failed, and the journal lasted only through its first volume. It had, however, "been most favorably received," as Silliman remarks, and it was felt that another journal of a similar type should be instituted. Such a suggestion was made by Col. Gibbs to Professor Silliman in 1817 and this led directly to the founding of the American Journal of Science in 1818 under the latter's editorship. Although the field of the Journal at the very beginning was made broad and inclusive it has always published many articles on mineralogical subjects. Three of its editors-in-chief have been eminent mineralogists, and without question it has been the most important single force in the development of this science in the country. More than 800 well-established mineral species have been described since the year 1800, of which approximately 150 have been from American sources. More than two-thirds of the articles describing these new American minerals have first appeared in the pages of the Journal. While the description of new species is not always the most important part of mineralogical investigation, still these figures serve to show the large part that the Journal has played in the growth of American mineralogy.

It is convenient to review the progress in Mineralogy according to the divisions formed by the different series, consisting of fifty volumes each, in which the Journal has

been published. These divisions curiously enough will be found to correspond closely to four quite definite phases through which mineralogical investigation in America has passed. The first series covered the years from 1817 to 1845. In looking through these volumes one finds a large number of mineralogical articles, the work of many contributors. The great majority of these papers are purely descriptive in character, frequently giving only general accounts of the mineral occurrences of particular regions. However, a number of articles dealing with more detailed physical and chemical descriptions of rare or new species also belong in this period. Among the mineralogists engaged at this time in the description of individual species, none was more indefatigable than Charles U. Shepard. He was graduated from Amherst College in 1824, at the age of twenty. In 1827 he became assistant to Professor Silliman in New Haven, continuing in this position for four years. Later he was a lecturer in natural history at Yale, and was at various times connected with Amherst College and the South Carolina Medical College at Charleston. His articles on mineralogy were very numerous. He assigned a large number of new names to minerals, although with the exception of some half dozen cases, these have later been shown to be varieties of minerals already known and described, rather than new species. In spite, however, of his frequent hasty and inaccurate decision as to the character of a mineral, his influence on the progress of mineralogy was marked. His great enthusiasm and ceaseless industry throughout a long life could not help but make a definite contribution to the science. His "Treatise on Mineralogy" will be spoken of in a later paragraph. He died in May, 1886, having published his last paper in the Journal in the previous September.

The first book on mineralogy published in America was that by Parker Cleaveland, professor of mathematics, natural philosophy, chemistry and mineralogy in Bowdoin College. The first edition was printed in 1816 and an exhaustive notice is given in the first volume of the Journal (1, 35, 308, 1818); a second edition followed in 1822. In his preface Cleaveland gives an interesting discussion concerning the two opposing European methods of classi-

fying minerals. The German school, led by Werner, classified minerals according to their external characters while the French school, following Haüy, put the emphasis on the "true composition." Cleaveland remarks that "the German school seems to be most distinguished by a technical and minutely descriptive language; and the French, by the use of accurate and scientific principles in the classification or arrangement of minerals." He, himself, tried to combine in a measure the two methods, basing the fundamental divisions upon the chemical composition and using the accurate description of the physical properties to distinguish similar species and varieties from each other.

Cleaveland's mineralogy was followed nearly twenty years later by the Treatise on Mineralogy by Charles U. Shepard already mentioned. The first part of this book was published in 1832. This contained chiefly an account of the natural history classification of minerals according to the general plan adopted by Mohs, the Austrian mineralogist. The second part of the book, which appeared in 1835, gave the description of individual species, the arrangement here being an alphabetical one throughout. Subsequent editions appeared in 1844, 1852 and 1857.

James Dwight Dana was graduated from Yale College in 1833 at the age of twenty. Four years later (1837) he published "The System of Mineralogy," a volume of 580 pages. The appearance of this book was an event of surpassing importance in the development of the science. The book, of course, depended largely upon the previous works of Haüy, Mohs, Naumann and other European mineralogists, but was in no sense merely a compilation from them. Dana, particularly in his discussion of mathematical crystallography, showed much original thought. He also proved his originality by proposing and using an elaborate system of classification patterned after those already in use in the sciences of botany and zoology. He later became convinced of the undesirability of this method of classification and abandoned it entirely in the fourth edition of the System, published in 1854, substituting for it the chemical classification which, in its essential features, is in general use to-day. The

System of Mineralogy started in this way in 1837, has continued by means of successive editions to be the standard reference book in the subject. The various editions appeared as follows: I, 1837; II, 1844; III, 1850; IV, 1854; V, 1868; VI, 1892 (by Edward S. Dana).

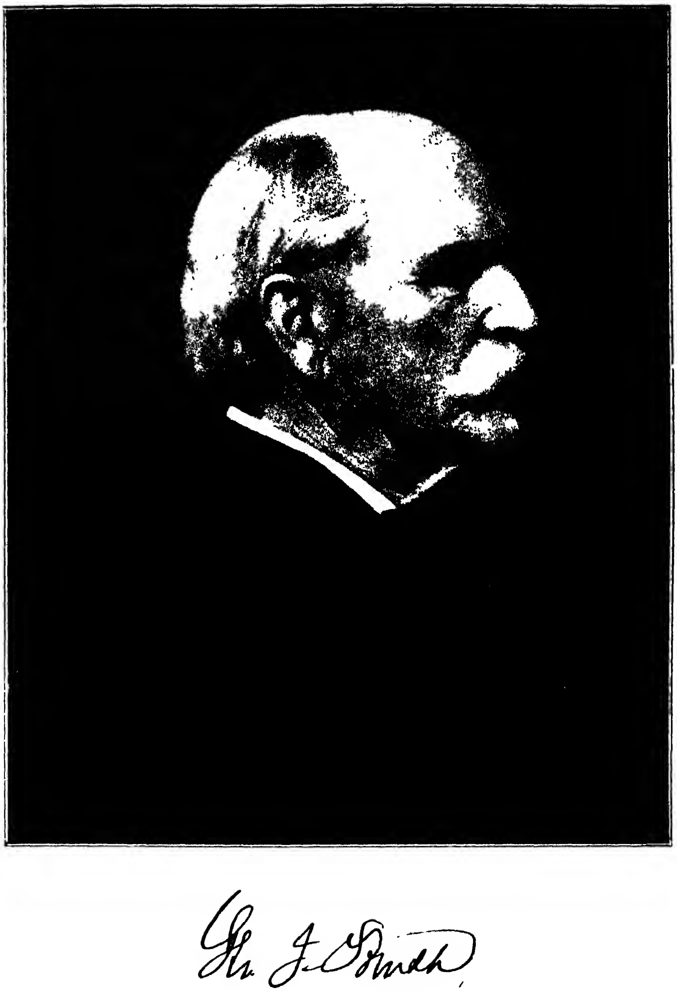
J. D. Dana also contributed numerous mineralogical articles to the first series of volumes of the Journal. It is interesting to note that they are chiefly concerned with the more theoretical aspects of the subject, in fact they constitute practically the only articles of such a character that appeared during this period. Among the subjects treated were crystallographic symbols, formation of twin crystals, pseudomorphism, origin of minerals in metamorphosed limestones, origin of serpentine, classification of minerals, etc.

The volumes of the Second Series of the Journal covered the years from 1846 through 1870. This period was characterized by great activity in the study of the chemical composition of minerals. A number of skilled chemists, notably J. Lawrence Smith, George J. Brush and Frederick A. Genth, began about 1850 a long series of chemical investigations of American minerals. Very few articles during this time paid much attention to the physical properties of the minerals under discussion, practically no description of optical characters was attempted, and only occasionally were the crystals of a mineral mentioned. J. D. Dana was almost the only writer who constantly endeavored to discover the fundamental characters and relationships in minerals. He published many articles in these years which were concerned chiefly with the classification and grouping of minerals, with similarities in the crystal forms of different species, with relations between chemical composition and crystal form, chemical formulas, mineral nomenclature, etc. The following titles give an idea of the character of the more important series of articles by him which belong to this category: On the isomorphism and atomic volume of some minerals (**9**, 220, 1850); various notes and articles on homœomorphism of minerals (**17**, 85, 86, 210, 430; **18**, 35, 131, 1854); on a connection between crystalline form and chemical constitution, with some inferences therefrom (**44**, 89, 252, 398, 1867).

A great many new mineral names were proposed between 1850 and 1870, a large number of which have continued to be well-recognized species. But there was also a tendency, which has not wholly disappeared even now, to base a mineral determination upon insufficient evidence, and to propose a new species with but little justification for it. In this connection a quotation from the introduction by J. D. Dana to the 3rd Supplement to the System of Mineralogy (4th edition) published in the Journal (**22**, page 246, 1856), will be of interest. He says:

"It is a matter of regret, that mineral species are so often brought out, especially in this country, without sufficient investigation and full description. It is not meeting the just demands of the science of mineralogy to say that a mineral has probably certain constituents, or to state the composition in a general way without a complete and detailed analysis, especially when there are no crystallographic characters to afford the species a good foundation. We have a right to demand that those who name species, should use all the means the science of the age admits of, to prove that the species is one that nature will own, for only such belong to science, and if enough of the material has not been found for a good description there is not enough to authorize the introduction of a new name in the science. The publication of factitious species, in whatever department of science, is progress not towards truth, but into regions of error; and often much and long labor is required before the science recovers from these backward steps."

J. Lawrence Smith was born in 1818 and died in 1883. He was a graduate of the University of Virginia and of the Medical College of Charleston and later spent three years studying in Paris. Shortly after the completion of his studies he went to Turkey as an advisor to the government of that country in connection with the growing of cotton there. During this time he investigated the emery mines of Asia Minor, and wrote a memoir upon them which was later published by the French Academy. He served as professor of chemistry in the University of Virginia and later held the same chair in the University of Illinois. He published a long series of papers on the chemical composition of minerals and meteorites, as well as on pure chemical subjects. Among the more notable



of his contributions are the "Memoir on Emery" (1850), a series of papers on the "Reëxamination of American Minerals" (1853) written with the collaboration of George J. Brush, and his "Memoir on Meteorites" (1855).

George J. Brush entered on his scientific career at the moment when science and scientific methods of research were just beginning to be appreciated in this country, and he soon became one of the leading pioneers in the movement. While his half century of active service was largely occupied by administrative duties in connection with the Sheffield Scientific School, his interest in mineralogy never flagged. His papers on mineralogical subjects number about thirty, all of which were published in the Journal. These began in 1849, even before his graduation from college, and continued until his last paper (in collaboration with S. L. Penfield) appeared in 1883. Three of the early papers were written with J. Lawrence Smith as noted above. These papers first set in this country the standard for thorough and accurate scientific mineral investigation. Later in life he was active in the development of the remarkable mineral locality at Branchville, Conn., and, with the collaboration of E. S. Dana, published in the Journal (1878-90) five important articles on its minerals. This locality, with the exception of the zinc deposits at Franklin Furnace, N. J., was the most remarkable yet discovered in this country. Nearly forty different mineral species were found there, of which nine (mostly phosphates) were new to science. There has certainly been no other series of descriptive papers on a mineralogical locality of equal importance published in this country.

In addition to publishing original papers, Brush did considerable editorial work in connection with the fourth (1854) and fifth (1868) editions of the System of Mineralogy and the Appendices to them. His Manual of Determinative Mineralogy, with a series of determinative tables adapted from similar ones by von Kobell, was first published in 1874. It was revised in 1878 and later rewritten by S. L. Penfield. This book did much to make possible the rapid and accurate determination of mineral species. Throughout his life, Brush was an enthusiastic

collector of minerals, building up the notable collection that now bears his name. Perhaps, however, his most important contribution to the development of mineralogy in America lay rather in his influence upon his many students. With his enthusiasm for accurate and painstaking investigation he was an inspiration to all who came in contact with him and his own field and science in general owes much to that influence.

Among the early mineralogists in this country, who were concerned in the chemical analyses of minerals, none accomplished more or better work than Frederick A. Genth. He was born in Germany in 1820 and lived in that country until 1848, when he came to the United States and settled in Philadelphia. He had studied in various German universities and worked under some of the most famous chemists of that time. His papers in mineralogy number more than seventy-five, in the great majority of which chemical analyses are given. He published fifty-four successive articles, the greater part of which appeared in the Journal, which were entitled Contributions to Mineralogy. In these he gave descriptions of more than two hundred different minerals, most of which were accompanied by analyses. He described more than a dozen new and well-established mineral species. He was especially interested in the rarer elements and many of his analyses were of minerals containing them. Especially interesting was his work with the tellurides, the species coloradoite, melonite and calaverite being first described by him. A long and important investigation was recorded on Corundum, "Its Alterations and Associate Minerals," published in the Proceedings of the American Philosophical Society in 1873 (**13**, 361). Dr. Genth died in 1893.

The period from 1860 until 1875 was not very productive in mineralogical investigations. The first ten volumes of the Third Series of the Journal, covering the years 1871-1876, contained mineralogical articles by only some fifteen different authors. But from that time on, the amount of work done and the number of investigators grew rapidly. With this increase in activity came also a decided change in the character of the work. The period between 1871 and 1895 can be characterized as one

in which all the various aspects of mineral investigation received more nearly equal prominence. While the chemical composition of minerals still held rightly its prominent place, the investigation of the crystallographic and optical characters and the relationships existing between all three were of much more frequent occurrence. Edward S. Dana commenced his scientific work by publishing in 1872 an article on the crystals of datolite which was probably the first American article concerned wholly with the description of the crystallography of a mineral. Samuel L. Penfield began his important investigations in 1877 and the first articles by Frank W. Clarke appeared during this period. The first edition of the Text Book of Mineralogy by Edward S. Dana with its important chapters on Crystallography and Optical Mineralogy was published in 1877 and his revision of the System of Mineralogy (sixth edition) appeared in 1892.

Unquestionably the foremost figure in American mineralogy during this period was that of Samuel L. Penfield. He embodied in an unusual degree the characters making for success in this science, for few investigators in mineralogy have shown, as he did, equal facility in all branches of descriptive mineralogy. He was a skilled chemist and possessed in a high degree that ingenuity in manipulation so necessary to a great analyst. He was also an accurate and resourceful crystallographer and optical mineralogist. His contributions to the science of mineralogy can be partially judged by the following brief summary of his work. He published over eighty mineralogical papers, practically all of which were printed in the Journal. These included the descriptions of fourteen new mineral species, the establishment of the chemical composition of more than twenty others, and the crystallization of about a dozen more. By a series of brilliant investigations he established the isomorphism between fluorine and the hydroxyl radical. He first enunciated the theory that the crystalline form of a mineral was due to the mass effect of the acid present rather than that of the bases. He contributed also a number of articles on the stereographic projection and its use in crystallographic investigations, devising a series of protractors and scales to make possible the rapid and accu-

rate use of this projection in solving problems in crystallography.

Penfield was born in 1856, was graduated from the Sheffield Scientific School in 1877 and immediately became an assistant in the chemical laboratory of that institution. At this time he, together with his colleague Horace L. Wells, made the analyses of the minerals from the newly discovered Branchville locality. He spent the years 1880 and 1881 in studying chemistry in Germany, returning to Yale as an instructor in mineralogy in the fall of 1881. Except for another semester in Europe at Heidelberg he continued as instructor and professor of mineralogy in the Sheffield Scientific School until his early death in 1906.

It is difficult to choose for mention the names of other investigators in Mineralogy during this period. Toward its end a great many writers contributed to the pages of the Journal, more than fifty different names being counted for the volumes **41** to **50** of the Third Series. Many of these are still living and still active in scientific research. Mention should be made of Frank W. Clarke, who contributed many important articles concerning the chemical constitution of the silicates. His work on the mica and zeolite groups is especially noteworthy. The work of W. H. Hillebrand, particularly in regard to his analytical investigations of the minerals containing the rarer elements, was of great importance. The name of W. E. Hidden should be remembered, because, with his keen and discriminating eye and active search for new mineral localities, he was able to make many additions to the science.

In glancing over the indices to the Journal the close interrelation of mineralogy to the other sciences is strikingly shown by the fact that so many scientists whose particular fields are along other lines have published occasional mineralogical papers. Frequently a young man has commenced with mineralogical investigations and then later been drawn definitely into one of these allied subjects. Men, who have won their reputation in chemistry, physics, and all the various divisions of geology, even that of palæontology, have all contributed articles distinctly mineralogical in character. For this

reason the number of American writers who have published what may be called casual papers on mineralogy is very great in comparison to the number of those who continue such publications over a series of years.

That the subject of meteorites is one which has been constantly studied by American mineralogists and petrographers is shown by the long list of papers concerning it that have been published in the Journal; it should, therefore, be considered briefly here. Many of these papers are short and of a general descriptive nature but others which give more fully the chemical, mineralogical and physical details are numerous. Among the earlier writers on this subject Benjamin Silliman, Jr., and C. U. Shepard should be mentioned. The latter was the first to recognize a new mineral in the Bishopville meteorite which he called chladnite. The same substance was afterwards found in a terrestial occurrence and was more accurately described by Kenngott under the name of enstatite. J. Lawrence Smith later showed that these two substances were identical. Smith did a large amount of important chemical work on meteorites. He was the first to note the presence of ferrous chloride in meteoric iron, the mineral being afterwards named lawrencite in his honor. The iron-chronium sulphide, daubreelite, was also first described by him. Other names that should be mentioned in this connection are those of A. W. Wright who studied the gaseous constituents of meteorites, G. F. Kunz, W. E. Hidden, A. E. Foote and H. A. Ward, all of whom published numerous descriptions of these bodies. Among the more recent workers in this field the names of G. P. Merrill and O. C. Farrington deserve especial mention.

The publication of the Fourth Series of the Journal began in 1896. Although the years since then have seen a great amount of very important work accomplished, the history of the period is fresh in the minds of all and as the majority of the active workers are still living and productive it seems hardly necessary to go into great detail concerning it. Twenty years ago it seemed to some mineralogists that the science could almost be considered complete. All the commoner minerals had certainly been discovered and exhaustively studied. Little

apparently was left that could be added to our knowledge of them. New occurrences would still be recorded, new crystal habits would be observed, and an occasional new and small crystal face might be listed, but few facts of great importance seemed undiscovered. This view was not wholly justified because new facts of interest and importance have continuously been brought forward, and the finding of new minerals does not appear to diminish in amount with the years. The work of the investigators on the United States Geological Survey along these lines is especially noteworthy.

This last of our periods, however, is chiefly signalized by a practically new development along the lines that might be characterized as experimental mineralogy. New ways have been discovered in which to study minerals. The important but hitherto baffling problems of their genesis, together with their relations to their surroundings, and to associated minerals, have been attacked by novel methods.

In this pioneer work that of the Geophysical Laboratory of the Carnegie Institution of Washington has been of the greatest importance. This laboratory was established in 1905 and, under the directorship of Arthur L. Day, a notable corps of investigators has been assembled and remarkable work already accomplished. While the field of investigation of the laboratory is broader than that of mineralogy, including much that belongs to petrography, vulcanology, etc., still the greater part of the work done can be properly classed as mineralogical in character and should be considered here. Because of its great value, however, it was felt that an authoritative, although necessarily, under existing conditions, a brief, account of it should be given. A concise summary of the objects, methods and results of the investigations of the laboratory has been kindly prepared by a member of its staff, Dr. R. B. Sosman, and is given later.

During the last few years another line of investigation has been opened by the discovery of the effect of crystalline structure upon X-rays. Through the refraction or reflection of the X-ray by means of the ordered arrangement of the particles forming the crystalline network, we are apparently going to be able to discover much con-

cerning the internal structure of crystals. And, partly through these discoveries, is likely to come in turn the solution of the hitherto insolvable mystery of the constitution of matter. Without doubt the multitudinous facts of mineralogy assembled during the past century by the painstaking investigation of a large number of scientists are destined to play a large part in the solution of this problem. Further, it does not seem too bold a prophecy to suggest, that the time will come when it will be possible to assemble all these unorganized facts that we know about minerals into a harmonious whole and that we shall be then able to formulate the underlying and fundamental principles upon which they all depend. These are the great problems for the future of mineralogical investigation.

IX

THE WORK OF THE GEOPHYSICAL LABORATORY OF THE CARNEGIE INSTITUTION OF WASHINGTON

By R. B. SOSMAN

THERE are three methods of approach to the great problem of rock formation. The first undertakes to reproduce by suitable laboratory experiments some of the observed changes in natural rocks. The second seeks to apply the principles of physical chemistry to a great body of carefully gathered statistics. The third method of attack is like the first in being a laboratory method, and like the second in seeking to apply existing knowledge to the association of minerals as found in rocks, but in its procedure differs widely from both. It consists of bringing together pure materials under measurable conditions, and thus in establishing by strictly quantitative methods the relations in which minerals can exist together under the conditions of temperature and pressure that have the power to affect such relations.

It is to this third method of investigation of the problems of the rocks that the Geophysical Laboratory has been devoted since its establishment in 1905. It has proved entirely practicable to make quantitative studies of the relations among the principal earth-forming oxides (silica, alumina, magnesia, lime, soda, potash, and the oxides of iron) over a very wide range of temperatures. The resources of physics have proved adequate to establish temperature with a high degree of precision and to measure the quantity of energy involved in the various reactions. The chemist has been able to obtain materials in a high degree of purity, and to follow out in detail the chemical relationships that exist among the

earth-forming oxides. The petrographic laboratory has been available for the comparison of synthetic laboratory products with the corresponding natural minerals.

It has also proved entirely practicable to extend the same methods of research to some of the principal ore minerals such as the sulphides of copper. Other information which is certain to be of ultimate economic value has also come out of the thorough study of the silicates, which are basic materials for the vast variety of industries which are classed under the name of ceramic industries. The best example of this is the facility with which the experience and the personnel of the laboratory has been adapted to the very important problem of manufacturing an adequate supply of optical glass for the needs of the United States in the present war.

It has further been possible to show within the last two years that rock formation in which volatile ingredients play a necessary and determining part can be completely studied in the laboratory with as much precision as though all the components were solids or liquids.

Along with the laboratory work on the formation of minerals and rocks has gone an increasing amount of field work on the activities of accessible volcanoes, such as Kilauea and Vesuvius, where the fusion and recrystallization of rocks on a large scale can be observed and studied.

There was once a time when the confidence of the laboratory in the capacity of physics and chemistry to solve geological problems was not shared by all geologists. There were some who were inclined to view with considerable apprehension the vast ramifications and complications of natural rock formation as a problem impossible of adequate solution in the laboratory. It is, therefore, a matter of satisfaction to all those who have participated in these efforts to see the evidences of this apprehension disappearing gradually as the work has progressed. A careful appraisement of the situation to-day, after ten years of activity, reveals the fact that the tangible grounds for anxiety about the *accessibility* of the problems which were confronted at first are now for the most part dissipated.

It will not be possible to review in detail the lines of

work sketched above. An outline of the synthetic work on systems of the mineral oxides and a paragraph on the volcano researches will perhaps suffice to indicate the general plan and purpose of the laboratory's work. It should be added that the results of many of the researches of the laboratory, detailed below, have been published in the pages of the Journal (see **21**, 89, 1906, and later volumes).

Mineral Researches.—The mineral studies include:

I. *One-component systems:* silica, with its numerous polymorphic forms and their relations to temperature and the conditions of rock formation; alumina; magnesia; and lime.

II. *Two-component systems:* silica-alumina, including sillimanite and related minerals; silica-magnesia, including the tetramorphic metasilicate $MgSiO_3$; silica-lime, including wollastonite; the alkali silicates, particularly with reference to their equilibria with carbon dioxide and with water; ferric oxide-lime; alumina-lime; alumina-magnesia, including spinel; and hematite-magnetite, a solid-solution series of an unusual type.

III. *Three-component systems:* silica-alumina-magnesia, completed but not yet published; silica-alumina-lime, complete, including the compounds that enter into the composition of portland cement; silica-magnesia-lime, completed but not yet published, including. however, published work on the diopside-forsterite-silica system, and on the $CaSiO_3$-$MgSiO_3$ series; and alumina-magnesia-lime.

IV. *Four components:* SiO_2-Al_2O_3-MgO-CaO: the incomplete system anorthite-forsterite-silica; SiO_2-Al_2O_3-CaO-Na_2O: the series of lime-soda feldspars (albite-anorthite), and the series nephelite (carnegieite)-anorthite; SiO_2-Al_2O_3-Na_2O-K_2O: the sodium-potassium nephelites.

V. *Five components:* SiO_2-Al_2O_3-MgO-CaO-Na_2O: the ternary system diopside-anorthite-albite (haplo-basaltic and haplo-dioritic magmas).

Fairly complete studies have also been made of the mineral sulphides of iron, copper, zinc, cadmium, and mercury, and the conditions controlling the secondary enrichment of copper sulphide ores are now being inves-

tigated. In connection with the sulphide investigations, the hydrated oxides of iron have been studied chemically and microscopically and the results will soon be ready for publication.

Throughout the work the mere accumulation of bodies of facts has been held to be secondary in importance to the development of new methods of attack and the evaluation of new general principles, and the specific problems studied have been selected from this point of view.

Volcano Researches.—A branch of the laboratory's work that is of general as well as petrological interest is the study of active volcanoes. Observations and collections have been made at Kilauea, Vesuvius, Etna, Stromboli, Vulcano, and (through the courtesy of the directors of the National Geographic Society) Katmai in Alaska. The great importance of gases in volcanicity is emphasized by all the studies. The active gases include hydrogen and water vapor, carbon monoxide and carbon dioxide, and sulphur and its oxides, as well as a variety of other compounds of lesser importance. The crater of Kilauea proves to be an active natural gas-furnace, in which reactions are continuously occurring among the gases, often resulting in making the lava basin hotter at the surface than it is at some depth. These reactions are being studied in the laboratory on mixtures of the pure constituent gases in known proportions, in order to lay the foundation for accurate interpretation and prediction concerning the gases as actually collected from the volcanoes themselves.

X

THE PROGRESS OF CHEMISTRY DURING THE PAST ONE HUNDRED YEARS

By HORACE L. WELLS and HARRY W. FOOTE

Introduction.

AS we look back to the time of the founding of the Journal in 1818, we see that the science of chemistry had recently made and was then making great advances. That the scientific men of those days were much impressed with what was being accomplished is well shown by the following statement made in an early number of the Journal (**3**, 330, 1821) by its founder in reviewing Gorham's Elements of Chemical Science. He says: "The present period is distinguished by wonderful mental activity; it might indeed be denominated as the intellectual age of the world. At no former period has the mind of man been directed at one time to so many and so useful researches."

A very remarkable revolution in chemical ideas had recently taken place. Soon after the discovery of oxygen by Priestley in 1774, and the subsequent discovery by Cavendish that water was formed by the combustion of hydrogen and oxygen, Lavoisier had explained combustion in general as oxidation, thus overthrowing the curious old phlogiston theory which had prevailed as the basis of chemical philosophy for nearly a century.

The era of modern chemistry had thus begun, and the additional views that matter was indestructible and that chemical compounds were of constant composition had been generally accepted at the beginning of the nineteenth century.

Dalton had announced his atomic theory in 1802, having based it largely upon the law of multiple proportions

which he had previously discovered, and he had begun to express the formulas for compounds in terms of atomic symbols.

In 1808 Gay-Lussac had discovered his law of gas combination in simple proportions,[1] a law of supreme importance in connection with the atomic theory, but neither he nor Dalton had seen this theoretical connection. Avogadro had understood it, however, and in 1811 had reached the momentous conclusion that all gases and vapors have equal numbers of molecules in equal volumes at the same temperature and pressure.

Davy in 1807 had isolated the alkali-metals, sodium and potassium, by means of electrolysis, thus practically dispelling the view that certain earthy substances might be elementary; and about four years later he had demonstrated that chlorine was an element, not an oxide as had been supposed previously, thus overthrowing Lavoisier's view that oxygen was the characteristic constituent of all acids.

At the time that our period of history begins, the atomic theory had been accepted generally, but in a somewhat indefinite form, since little attention had been paid to Avogadro's principle, and since Dalton had used only the principle of greatest simplicity in writing the formulas of compounds, considering water as HO and ammonia NH, for example. At this time, however, Berzelius for ten or fifteen years had been devoting tremendous energy to the task of determining the atomic weights of nearly all of the elements then known by analyzing their compounds. He had confirmed the law of multiple proportions, accepted the atomic theory, and utilized Avogadro's principle, and it is an interesting coincidence that his first table of atomic weights was published in the year 1818.

An interesting account of the views on chemistry held at about that time was published in the Journal by Denison Olmsted (**11**, 349, 1826; **12**, 1, 1827), who had recently become professor of natural philosophy in Yale College.

The most illustrious European chemists of that time were Berzelius of Sweden, Davy of England, and Gay-Lussac of France, and the curious circumstance may be

mentioned that all three of them and also Benjamin Silliman, the founder of the Journal, were born within a period of eight months in 1778-1779.

In this country Robert Hare of Philadelphia and Benjamin Silliman were undoubtedly the most prominent chemists of those days. Hare is best known for his invention of the compound blowpipe, but his contributions to the Journal were very numerous, beginning almost with the first volume and continuing for over thirty years. Among the first of these contributions was a most vigorous but well-merited attack upon a Doctor Clark of Cambridge, England, who had copied his invention without giving him proper credit. He begins (**2**, 281, 1820) by saying: "Dr. Clark has published a book on the gas blowpipe in which he professes a sincere desire to render everyone his due. That it would be difficult for the conduct of any author to be more discordant with these professions, I pledge myself to prove in the following pages."

Hare also invented a galvanic battery which he called a "deflagrator," consisting of a large number of single cells in series. With this, using carbon electrodes, he was able to obtain a higher temperature than with his oxy-hydrogen blowpipe. He was the first to apply galvanic ignition to blasting (**21**, 139, 1832), and he first carried out electrolyses with the use of mercury as the cathode (**37**, 267, 1839). In this way he prepared metallic calcium and other metals from solutions of their chlorides, while the principle employed by him has in recent times been used as the basis of a very important process for manufacturing caustic potash and soda.

Silliman, who had become an intimate friend of Hare during two periods of chemical study under Woodhouse in Philadelphia in 1802-1804, and who soon afterwards spent fourteen months as a student abroad, chiefly in England and Scotland, took a broad interest in science and gave much attention to geology as well as to chemistry. In spite of this divided interest and his work as a teacher, popular scientific lecturer, and editor, he found time for a surprising amount of original chemical work. For instance, using Hare's deflagrator, he showed that carbon was volatilized in the electric arc (**5**, 108, 1822);

he was the first in this country to prepare hydrofluoric acid (6, 354, 1823), and he first detected bromine in one of our natural brines (18, 142, 1830).

Atomic Weights.

As soon as the atomic theory was accepted, the relative weights of the atoms became a matter of vital importance in connection with formulas and chemical calculations. In advancing his theory, Dalton had made some very rough atomic weight determinations, and it has been mentioned already that Berzelius, at the time that our historical period begins, was engaged in the prodigious task of accurately determining these constants for nearly all the known elements. It is recorded that he analyzed quantitatively no less than two thousand compounds in connection with this work during his career. His table of 1818 has proved to be remarkably accurate for that pioneer period, and it indicates his remarkable skill as an analyst.

It is to be observed that Berzelius in this early table made use of Avogadro's principle in connection with elements forming gaseous compounds, and thus obtained correct formulas and atomic weights in such cases, but that in many instances his atomic weights and those now accepted bear the relation of simple multiples to one another, because he had then no means of deciding upon the formulas of many compounds except the rule of assumed simplicity. For example, the two oxides of iron now considered to be FeO and Fe_2O_3 he regarded as FeO_2 and FeO_3, knowing as he did that the ratio of oxygen in them was 2 to 3, and believing that a single atom of iron in each was the simplest view of the case, so that as the consequence of these formulas the atomic weight of iron was then considered to be practically twice as great in its relation to oxygen as at present.

These old atomic weights of Berzelius, used with the corresponding formulas, were just as serviceable for calculating compositions and analytical factors as though the correct multiples had been selected. As time went on, the true multiples were gradually found from considerations of atomic heats, isomorphism, vapor densities,

the periodic law, and so on, and suitable changes were made in the chemical formulas.

Berzelius used 100 parts of oxygen as the basis of his atomic weights, a practice which was generally followed for several decades. Dalton, however, had originally used hydrogen as unity as the basis, and this plan finally came into use everywhere, as it seemed to be more logical and convenient, because hydrogen has the smallest atomic weight, and also because the atomic weights of a number of common elements appeared to be exact multiples of that of hydrogen, thus giving simpler numbers for use in calculations.

Within a few years a slight change has been made by the adoption of oxygen as exactly 16 as the basis, which gives hydrogen the value of 1·008.

As early as 1815, Prout, an English physician, had advanced the view that hydrogen is the primordial substance of all the elements, and consequently that the atomic weights are all exact multiples of that of hydrogen. This hypothesis has been one of the incentives to investigations upon atomic weights, for it has been found that these constants in the cases of a considerable number of the elements are very close to whole numbers when based upon hydrogen as unity, or even still closer when based upon oxygen as 16.

With our present knowledge Prout's hypothesis may be regarded as disproved for nearly all the elements whose atomic weights have been accurately determined, but the close or even exact agreement with it in a few cases is still worthy of consideration. There is an interesting letter from Berzelius to B. Silliman, Jr., in the Journal (**48**, 369, 1845) in which Berzelius considers the theory entirely disproved.

For a long time entire reliance was placed upon the atomic weights obtained by Berzelius, but it came to be observed that the calculation of carbon from carbon dioxide appeared to give high results in certain cases, so that doubt arose as to the accuracy of Berzelius's work. Consequently in 1840 Dumas, assisted by his pupil Stas, made a new determination of the atomic weight of carbon, and found that the number obtained by Berzelius, 12·12, was slightly too large. Subsequently Dumas determined

more than twenty other atomic weights, but this great amount of work did not bring about any considerable improvement, for it appears that Dumas did not greatly excel Berzelius in accuracy, and that the latter had made one of his most noticeable errors in connection with carbon.

Soon after assisting Dumas in the work upon carbon, Stas began his very extensive and accurate, independent determinations, leading to the publication of a book in 1867 describing his work. Stas made many improvements in methods by the use of great care in purifying the substances employed, and especially by using large quantities of material in his determinations, thus diminishing the proportional errors in weighing. His results, which dealt with most of the common elements, were accepted with much confidence by chemists everywhere.

Stas reached the conclusion that there could be no real foundation for Prout's hypothesis, since so many of his atomic weights varied from whole numbers, and this opinion has been generally accepted.

The first accurate atomic weight determination published in the Journal was that by Mallett on lithium (**22**, 349, 1856; **28**, 349, 1859), showing a result almost identical with that accepted at the present time. Johnson and Allen's determination (**35**, 94, 1863) on the rare element cæsium was carried out with extraordinary accuracy. Lee, working with Wolcott Gibbs, made good determinations on nickel and cobalt (**2**, 44, 1871). The work of Cooke on antimony (**15**, 41, 107, 1878) was excellent.

Concerning the more recent work published elsewhere than in the Journal, attention should be called particularly to the investigations that have been carried on for the past twenty-five years by Richards and his associates at Harvard University. Richards has shown masterly ability in the selection of methods and in avoiding errors. His results have displayed such marvelous agreements among repeated determinations by the same and by different processes as to inspire the greatest confidence. His work has been very extensive, and it is a great credit to our country that this atomic weight work, so superior to all that has been previously done, is being carried out here.

It may be mentioned that for a number of years the decision in regard to the atomic weights to be accepted has been in the hands of an International Committee of which our fellow countryman F. W. Clarke has been chairman. In connection with this position and previously, Clarke has done valuable service in re-calculating and summarizing atomic weight determinations.

Analytical Chemistry.

Analysis is of such fundamental importance in nearly every other branch of chemical investigation that its development has been of the utmost importance in connection with the advancement of the science. It attained, therefore, a comparatively early development, and one hundred years ago it was in a flourishing condition, particularly as far as inorganic qualitative and gravimetric analysis were concerned. There is no doubt that Berzelius, whose atomic weight determinations have already been mentioned, surpassed all other analysts of that time in the amount, variety, and accuracy of his gravimetric work. He lived through three decades of our period, until 1848.

During the past century there has been constant progress in inorganic analysis, due to improved methods, better apparatus and accumulated experience. An excellent work on this subject was published by H. Rose, a pupil of Berzelius, and the methods of the latter, with many improvements and additions by the author and others, were thus made accessible. Fresenius, who was born in 1818, did much service in establishing a laboratory in which the teaching of analytical chemistry was made a specialty, in writing text-books on the subject and in establishing in 1862 the "Zeitschrift für analytische Chemie," which has continued up to the present time.

Besides Berzelius, who was the first to show that minerals were definite chemical compounds, there have been many prominent mineral analysts in Europe, among whom Rammelsberg and Bunsen may be mentioned, but there came a time towards the end of the nineteenth century when the attention of chemists, particularly in Germany, was so much absorbed by organic chemistry that

mineral analysis came near becoming a lost art there. It was during that period that an English mineralogist, visiting New Haven and praising the mineral analyses that were being carried out at Yale, expressed regret that there appeared to be no one in England, or in Germany either, who could analyze minerals.

The best analytical work done in this country in the early part of our period was chiefly in connection with mineral analysis, and a large share of it was published in the Journal. Henry Seybert, of Philadelphia, in particular, showed remarkable skill in this direction, and published numerous analyses of silicates and other minerals, beginning in 1822. It was he who first detected boric acid in tourmaline (**6**, 155, 1822), and beryllium in chrysoberyl (**8**, 105, 1824). His methods for silicate analyses were very similar to those used at the present time.

J. Lawrence Smith in 1853 described his method for determining alkalies in minerals (**16**, 53), a method which in its final form (**1**, 269, 1871) is the best ever devised for the purpose. He also described (**15**, 94, 1853) a very useful method, still largely used in analytical work, for destroying ammonium salts by means of aqua regia. Carey Lea (**42**, 109, 1866) described the well-known test for iodides by means of potassium dichromate. F. W. Clarke (**49**, 48, 1870) showed that antimony and arsenic could be quantitatively separated from tin by the precipitation of the sulphides in the presence of oxalic acid. In 1864 Wolcott Gibbs (**37**, 346) began an important series of analytical notes from the Lawrence Scientific School, and he worked out later many difficult analytical problems, particularly in connection with his extensive researches upon the complex inorganic acids.

From 1850 on, Brush and his students made many important investigations upon minerals, and from 1877 Penfield (**13**, 425), beginning with an analysis of a new mineral from Branchville, Connecticut, described by Brush and E. S. Dana, displayed remarkable skill and industry in this kind of work. Both of the writers of this article were fortunate in being associated with Penfield in some of his researches upon minerals and one of us began as he did with the Branchville work It is

probably fair to say that Penfield did the most accurate work in mineral analysis that has ever been accomplished, and that he was similarly successful in crystallography and other physical branches of mineralogy.

The American analytical investigations that have been mentioned were all published in the Journal, with the exception of a part of Gibbs's work. Many other American workers at mineral analysis might be alluded to here, but only the excellent work of a number of chemists in the United States Geological Survey will be mentioned. Among these Hillebrand deserves particular praise for the extent of his investigations and for his careful researches in improving the methods of rock analysis.

To our own Professor Gooch especial praise must be accorded for the very large number of analytical methods that have been devised, or critically studied, by him and his students, and for the excellent quality of this work. The publications in the Journal from his laboratory began in 1890 (**39**, 188), and the extraordinary extent of this work is shown by the fact that the three hundredth paper from the Kent Laboratory appeared in May, 1918. These very numerous and important investigations have been of great scientific and practical value, and they have formed a striking feature of the Journal for nearly 30 years. In 1912 Gooch published his "Methods in Chemical Analysis," a book of over 500 pages, in which the work in the Kent Chemical Laboratory up to that time was concisely presented. Among the many workers who have assisted in these investigations, P. E. Browning, W. A. Drushel, F. S. Havens, D. A. Kreider, C. A. Peters, I. K. Phelps and R. G. Van Name are particularly prominent. Besides many other useful pieces of apparatus, the perforated filtering crucible was devised by Gooch, and this has brought his name into everyday use in all chemical laboratories.

Volumetric analysis was originated by Gay-Lussac, who described a method for chlorimetry in 1824, for alkalimetry in 1828, and for the determination of silver and chlorides in 1832. Margueritte devised titrations with potassium permanganate in 1846, while Bunsen, not far from the same time, introduced the use of iodine and sulphur dioxide solutions for the purpose of determining

many oxidations and reductions. We owe to Mohr some improvements in apparatus and a German text-book on the subject, while Sutton wrote an excellent English work on volumetric analysis, of which many editions have appeared.

While volumetric analysis began to be used less than one hundred years ago, its applications have been gradually extended to a very great degree, and it is not only exceedingly important in investigations in pure chemistry, but its use is especially extensive in technical laboratories where large numbers of rapid analyses are required.

Not a few volumetric methods have been devised or improved in the United States, but mention will be made here only of Cooke's important method for the determination of ferrous iron in insoluble silicates, published in the Journal (**44**, 347, 1867); to Penfield's method for the determination of fluorine in 1878; and to the more recent general method of titration with an iodate in strong hydrochloric acid solutions, due to L. W. Andrews, a number of applications of which have been worked out in the Sheffield Laboratory.

A considerable amount of work with gases had been done by Priestley, Scheele, Cavendish, Lavoisier, Dalton, Gay-Lussac, and others before our hundred-year period began. Cavendish, about 1780, had analyzed atmospheric air with remarkable accuracy, and had even separated the argon from it and wondered what it was, and later Gay-Lussac had shown great skill in the study of gas reactions. During our period gas analysis has been further developed by many chemists. Bunsen, in particular, brought the art to a high degree of perfection in the course of a long period beginning about 1838, the last edition of his "Methods of Gas Analysis" having been published in 1877.

Important devices for the simplification of gas-analysis in order that it might be used more conveniently for technical purposes have been introduced by Orsat in France and by Winkler, Hempel and Bunte in Germany.

It appears that our countryman Morley has surpassed all others in accurate work with gases in connection with his determinations of the combining weights and

volumes of hydrogen and oxygen about the year 1891. Some of his publications have appeared in the Journal (**30**, 140, 1885; **41**, 220, 1891; and others).

Electrolytic analysis, involving the deposition of metals, or sometimes of oxides, usually upon a platinum electrode, was brought into use in 1865 by Wolcott Gibbs through an article published in the Journal (**39**, 58, 1865). He there described the electrolytic precipitation of copper and of nickel by the methods still in use. The application of the process has been extended to a number of other metals, and it has been largely employed, particularly in technical analyses. Important investigations and excellent books on this subject have been the contributions of Edgar F. Smith of the University of Pennsylvania, and the useful improvement, the rotating cathode, was devised by Gooch and described in the Journal (**15**, 320, 1903).

General Inorganic Chemistry.

The Chemical Symbols.—It is to Berzelius that we owe our symbols for the atoms, derived usually from their Latin names, such as C for carbon, Na for sodium, Cl for chlorine, Fe for iron, Ag for silver, and Au for gold. We owe to him also the use of small figures to show the number of atoms in a formula, as in N_2O_5. This was a marked improvement over the hieroglyphic symbols proposed by Dalton, which were set down as many times as the atoms were supposed to occur in formulas, forming groups of curious appearance, but in some respects not unlike some of our modern developed formulas. The advantages of Berzelius's symbols were their simplicity, legibility, and the fact that they could be printed without the need of special type. It is true that at a later period Berzelius used certain symbols with horizontal lines crossing them to represent double atoms, and that these made some difficulty in printing. It should be mentioned also that Berzelius at one time made an effort to simplify formulas by placing dots over other symbols to represent oxygen, and commas to represent sulphur atoms. Examples of these are:

$\dot{\mathrm{Ca}}\dddot{\mathrm{S}}$, calcium sulphate; $\overset{,,}{\mathrm{Fe}}$, iron disulphide

This form of notation was quite extensively employed for a time, especially by mineralogists, but it was entirely abandoned later.

It is interesting to notice that Dalton, who lived until 1844, to reach the age of 78, differed from other chemists in refusing to accept the letter-symbols of Berzelius. In a letter written to Graham in 1837 he said: "Berzelius's symbols are horrifying. A young student in chemistry might as soon learn Hebrew as to make himself acquainted with them. They appear like a chaos of atoms . . . and to equally perplex the adepts of science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory."

This forcibly expressed opinion was apparently tinged with self-esteem, but there is no doubt that Dalton was sincere in believing that the atoms were best represented by his circular symbols, because, as is well known, he thought that all the atoms were spherical in form, and it is evident that circles give the proper picture of spherical objects. At the present time some insight as to the structure of atoms is being gained, and it appears possible that the time may come when pictures of their external appearance that are not wholly imaginary may be made.

Changes in Formulas.—Even before the year 1826, Berzelius displayed great skill in arriving at many formulas that agree with our present ones, for example, H_2O for water, $ZnCl_2$ for zinc chloride, N_2O_5 for nitric acid (anhydride), CaO for calcium oxide, CO and CO_2 for the oxides of carbon, and many others. But at the same period other authorities, especially Gay-Lussac in France and Gmelin in Germany, on account of a lack of appreciation for Avogadro's principle and for other reasons, such as the use of symbols to represent combining weights rather than atoms, were using different formulas for some of these compounds, such as HO, ZnCl and NO_5, so that their formulas for many of the compounds of hydrogen, chlorine, nitrogen and several other elements differed from those of Berzelius. The employment of different formulas involved the use of different atomic or combining weights. For example, with the formula H_2O for water the composition by weight requires the

ratio 1 to 16 for the weights of the hydrogen and oxygen atoms, while with HO the ratio is 1 to 8.

Berzelius attempted to bring about greater uniformity in formulas and atomic weights by making changes in his table of atomic weights published in 1826. He practically doubled the relative atomic weights of hydrogen, chlorine, nitrogen, and of the other elements that gave twice as many atoms in his formulas as in those of others, and at the same time he wrote the symbols of these elements with a bar across them to indicate that they represented double atoms. For example, he wrote:

$$\bar{H}O \quad Zn\bar{C}l \quad \bar{N}O_3$$

instead of

$$H_2O,\ ZnCl_2\ N_2O_3$$

This appears to have been an unfortunate concession to the views of others on the part of Berzelius, for the barred symbols were not generally adopted, partly on account of difficulties in printing, and the great achievement in theory made by him was lost sight of for a long period of time.

The Law of Atomic Heats.—In 1819, Dulong and Petit of France, from experiments upon the specific heats of a number of solid elementary substances, came to the conclusion that the atoms of simple substances have equal capacities for heat, or in other words, that the specific heats of elements multiplied by their atomic weights give a constant called the atomic heat. For instance, the specific heats of sulphur, iron, and gold have been given as 0·2026, 0·110, and 0·0324, while their atomic weights are about 32, 56, and 197, respectively; hence the atomic heats obtained by multiplication are 6·483, 6·116, and 6·383.

Further investigations showed that the atomic heats display a considerable variation. Those of carbon, boron, beryllium, and silicon are very low at ordinary temperatures. although they increase and approach the usual values at higher temperatures. More recent work has shown, however, that the specific heats of other elements vary greatly with the temperature, almost disappearing at the temperature of liquid hydrogen, and hence possibly disappearing entirely at the absolute zero, where

the electrical resistance of the metals appears to vanish likewise.

It has been found that most of the solid elements near ordinary temperatures give atomic heats that are approximately 6·4. Berzelius applied the law in fixing a number of atomic weights, and its importance for this purpose is still recognized.

It may be mentioned here that two well-known Yale men, W. G. Mixter and E. S. Dana, while students in Bunsen's laboratory at Heidelberg in 1873, made determinations of the specific heats of boron, silicon, and zirconium. This was the first determination of this constant for zirconium, and it was consequently important in establishing the atomic weight of that element.

Isomorphism and Polymorphism.—Mitscherlich observed in 1818 that certain phosphates and arsenates have the same crystalline form, and afterwards he reached the conclusion that identity in form indicates similarity in composition in connection with the number of atoms and their arrangement. This law of isomorphism was of much assistance in the establishment of correct formulas and consequently of atomic weights. For instance, since the carbonates of barium, strontium, and lead crystallize in the same form, the oxides of these metals must have analogous formulas. From such considerations Berzelius was able to make several improvements in his atomic weight table of 1826.

Mitscherlich was the first to observe two forms of sulphur crystals, and from this and other cases of dimorphism or of polymorphism it became evident that analogous compounds were not necessarily always isomorphous, a circumstance which has restricted the application of the law to some extent.

Besides its application in fixing analogous formulas, the law of isomorphism has come to be of much practical use in the understanding and simplification of the formulas for minerals, for these natural crystals very often contain several isomorphous compounds in varying proportions, and an understanding of this "isomorphous replacement," as it is called, makes it possible to deduce simple general formulas for them.

In some cases isomorphism takes place to a greater or less extent between substances which are not chemically similar, and this brings about a variation in composition which at times has caused confusion. For instance, the mineral pyrrhotite has a composition which usually varies between Fe_7S_8 and $Fe_{11}S_{12}$, and both these formulas have been assigned to it. It was recently shown by Allen, Crenshaw and Johnston in the Journal (**33**, 169, 1912) that this is a case where the compound FeS is capable of taking up various amounts of sulphur isomorphously.

The idea of solid solution was advanced by van't Hoff to explain the crystallization of mixtures, including cases of evident isomorphism. This view has been widely accepted, and it has been particularly useful in cases where isomorphism is not evident. Solid solution between metals has been found to be exceedingly common, many alloys being of this character. A case of this kind was observed by Cooke and described in the Journal (**20**, 222, 1855). He prepared two well-crystallized compounds of zinc and antimony to which he gave the formulas Zn_3Sb and Zn_2Sb, but he observed that excellent crystals of each could be obtained which varied largely in composition from these formulas. As the two compounds were dissimilar in their formulas and crystalline forms, Cooke assumed that isomorphism was impossible and concluded "that it is due to an actual perturbation of the law of definite proportions, produced by the influence of mass." We should now regard this as a case of solid solution.

A Lack of Confidence in Avogadro's Principle.—One reason why chemists were so slow in arriving at the correct atomic weights and formulas was a partial loss of confidence in Avogadro's principle. About 1826 the young French chemist Dumas devised an excellent method for the determination of vapor densities at high temperatures, and his results and those of others showed some discrepancies in the expected densities. For example, the vapor density of sulphur was found to be about three times too great, that of phosphorus twice too great, that of mercury vapor and that of ammonium

chloride only about half large enough to correspond to the values expected from analogy and other considerations. Thus, one volume of oxygen with two volumes of hydrogen make two volumes of steam, but only one-third of a volume of sulphur vapor was found to unite with two volumes of hydrogen to make two volumes of hydrogen sulphide. Berzelius saw clearly that the results pointed to the existence of such molecules as S_6, P_4, and Hg_1, but it was not generally realized in those days that Avogadro's rule is fundamentally reliable, and Berzelius himself appears to have lost confidence in it on account of these complications, for he did not apply Avogadro's principle to decisions about atomic weights except in the cases of substances gaseous at ordinary temperatures.

Electro-chemical Theories. — The observation was made by Nicholson and Carlisle in 1800 that water was decomposed into its constituent gases by the electric current. Then in 1803 Berzelius and Hisinger found that salts were decomposed into their bases and acids by the same agency, and in 1807 Davy isolated potassium, sodium, and other metals afterwards, by a similar decomposition. Since those early times a vast amount of attention has been paid to the relation of electricity to chemical changes, a relation that is evidently of great importance from the fact that while electric currents decompose chemical compounds, these currents, on the other hand, are produced by chemical reactions.

Berzelius was particularly prominent in this direction, and in 1819 he published an elaborate electro-chemical theory. He believed that atoms were electrically polarized, and that this was the cause of their combination with one another. He extended this idea to groups of atoms, particularly to oxides, and regarded these groups as positive or negative, according to the excess of positive or negative electricity derived from their constituent atoms and remaining free. He thus arrived at his dualistic theory of chemical compounds, which attained great prominence and prevailed for a long time in chemical theory. According to this idea, each compound was supposed to be made up of a positive and a

negative atom or group of atoms. For example, the formulas for potassium nitrate, calcium carbonate, and sulphuric acid corresponded to $K_2O.N_2O_5$, $CaO.CO_2$ and $H_2O.SO_3$ where we now write KNO_3, $CaCO_3$ and H_2SO_4, and the theory was extended to embrace organic compounds also.

The eminent English chemist and physicist Faraday announced the important law of electro-chemical equivalents in 1834. This law shows that the quantities of elements set free by the passage of a given quantity of electricity through their solutions correspond to the chemical equivalents of those elements. Faraday made a table of the equivalents of a number of elements, regarding them important in connection with atomic weights, but at that time no sharp distinction was usually made between equivalents and atomic weights, and it was not fully realized that one atom of a given element may be the electrical equivalent of several atoms of another.

Faraday's law, which is still regarded as fundamentally exact, has been of much practical use in the measurement of electric currents and in calculations connected with electro-chemical processes. In discussing his experiments, Faraday made use of several new terms, such as "electrolyte" for a substance which conducts electricity when in solution, and is thus "electrolyzed," "electrode," "anode," and "cathode," terms that have come into general use, and finally "ions" for the particles that were supposed to "wander" towards the electrodes to be set free there.

This term "ion" remained in comparative obscurity for more than half a century, when it was brought into great prominence among chemists by Arrhenius in connection with the ionic theory.

Cannizzaro's Ideas.—Up to about 1869 chaos reigned among the formulas used by different chemists. Various compound radicals and numerous type-formulas were employed, dualistic and unitary formulas of several kinds were in use, but the worst feature of the situation was the fact that more than one system of atomic weights was in vogue, so that water might be written

HO, HO, or H_2O

and similar discrepancies might appear in nearly all formulas containing elements of different valencies. In 1858, however, an article by the Italian chemist Cannizzaro appeared in which the outlines of a course in chemical philosophy were presented. This acquired wide circulation in the form of a pamphlet at a chemical convention somewhat later, and it dealt so clearly and ably with Avogadro's principle, Dulong and Petit's law, and other points in connection with formulas that it led to a rapid and almost universal reform among those who were using unsatisfactory formulas.

At about this time also the dualistic formulas of Berzelius were generally abandoned, and hydrogen came to be regarded as the characteristic element of all acids. For instance, $CaO.SO_3$, called "sulphate of lime," came to be written $CaSO_4$ and was called "calcium sulphate," and while it had been shown as early as 1815 by Davy that "iodic acid," I_2O_5, showed no acid reaction until it was combined with water, the accumulation of similar facts led to the formulation of sulphuric acid as H_2SO_4 instead of SO_3 or $H_2O.SO_3$, and that of other "oxygen acids" in a similar way. As a necessary consequence of this view of acids, the bases came to be regarded as compounds of the "hydroxyl" group, OH. Therefore the formula for caustic soda came to be written NaOH instead of $Na_2O.H_2O$, and so on.

The Periodic System of the Elements.—The periodicity of the elements in connection with their atomic weights was roughly grasped by Newlands in England, who announced his "law of octaves" in 1863. This was at the time when the atomic weights were being modified and their numerical relations properly shown. The subject was worked out more fully by L. Meyer in Germany a little later, but it was most clearly and elaborately presented by the Russian chemist Mendeléeff in 1869.

In order that this subject may be explained to some extent Mendeléeff's table is given here, with the addition of the recently discovered elements and some other modifications.

Groups	I A	I B	II A	II B	III A	III B	IV A	IV B	V A	V B	VI A	VI B	VII A	VII B	VIII A	VIII B
Typical compounds	R_2O RCl RH	—	RO RCl_2 (RH_2)	—	—	R_2O_3 RCl_3 (RH_3)	—	RO_2 RCl_4 (RH_4)	—	R_2O_5 RCl_3 RH_3	—	RO_3 RCl_2 RH_2	— —	R_2O_7 RCl RH	(RO_4) — —	— R R
Series 1														HYDROGEN 1·008		HELIUM 3·99
2	Lithium 6 94			Beryllium 9·1		*Boron* 11·0		*Carbon* 12·00		NITROGEN 14·01		OXYGEN 16·00		FLUORINE 19·0		NEON 20·2
3	Sodium 23·00			Magnesium 24·32		Aluminium 27·1		*Silicon* 28 3		*Phosphorus* 31·04		*Sulphur* 32·07		CHLORINE 35·46		ARGON 39·88
4	Potassium 39·10		Calcium 40·07		Scandium 44·1		Titanium 48·1		Vanadium 51·0		Chromium 52·0		Manganese 54·93		Iron 55·84 Cobalt 58·97 Nickel 58·68	
5		Copper 63·57		Zinc 65·37		Gallium 69·9		Germanium 72·5		Arsenic 74·96		*Selenium* 79·2		*Bromine* 79·92		KRYPTON 82·92
6	Rubidium 85·43		Strontium 87·63		Yttrium 89·0		Zirconium 90·6		Niobium 93·5		Molybdenum 96·0		— 100		Ruthenium 101·7 Rhodium 102·9 Palladium 106·7	
7		Silver 107·88		Cadmium 112·40		Indium 114·8		Tin 119·0		Antimony 120 2		Tellurium 127·5		*Iodine* 126·92		XENON 130·2
8	Cæsium 132·81		Barium 137·37		Lanthanum 139·0 to* Lutecium 174·0		(Cerium) 140·25 (Lutecium) 174·0		Tantalum 181·5		Tungsten 184·0		— 188		Osmium 190·9 Iridium 193·1 Platinum 195·2	
9		Gold 197·2		Mercury 200·6		Thallium 204·0		Lead 207·10		Bismuth 208·0		—		—		NITON 222·4
10	—		Radium 226·4		—		Thorium 232·4		—		Uranium 238·5					

* Rare-Earth Metals:							
Lanthanum, 139·0	Cerium, 140·25	Praseodymium, 140·6	Neodymium, 144·3	———,	Samarium, 150·4	Europeum, 152·0	———,
Gadolinium, 157·3	Terbium, 159·2	Dysprosium, 162·5	Holmium, 163·5	Erbium, 167·7	Thulium, 168·5	Ytterbium, 172·0	Lutecium, 174·0

NOTE.—Distinctions in printing: GASEOUS ELEMENTS, *Other non-metallic elements*, metallic elements. The heavy line encloses approximately the acid-forming elements.

In this table the elements arranged in the order of their atomic weights fall into eight groups where the known oxides progress regularly, with the exception of two or three elements, from R_2O in Group I to R_2O_7 in Group VII, while in Group VIII two oxides (of ruthenium and osmium) are known which carry the progression to RO_4.

It was pointed out by Mendeléeff that, with the exception of series 1 and 2 at the top of the table, the alternate members of the groups show particularly close relationships. These subordinate groups, marked A and B, in most cases show remarkable analogies and gradations in their properties, for example, in the alkali-metals from lithium to cæsium, and in the halogens from fluorine to iodine. The two divisions of a group do not usually show very close relations to each other, except in their valency, and they even display, in several instances, opposite gradations in chemical activity in the order of their atomic weights. For instance, cæsium stands at the electro-positive end, while gold stands at the electro-negative end of its subordinate group. The difference between the two divisions is very great in Groups VI and VII, but it is extreme in Group VIII, where heavy metals are on one side and inactive gases on the other. Many authorities separate these gases into a "Group O" by themselves at the left-hand side of the table, but this does not change their relative positions, and the plan may be objected to on the ground that many vacant places are thus left in the groups VIII and O.

The periodic law has been useful in rectifying certain atomic weights. At the outset Mendeléeff was obliged to change beryllium from 14·5 (assuming Be_2O_3) to 9 (assuming BeO), and later the atomic weights of indium and uranium were changed to make them fit the system. All of these changes have been confirmed by physical means.

Mendeléeff found a number of vacant places in his table, and was thus able to render further service to chemical science by predicting the properties of undiscovered elements, and his predictions were very closely confirmed by the later discovery of scandium, gallium, and germanium. The table indicates that there are still

two undiscovered elements below manganese and probably two more among the rare-earth metals. The interesting observation has just recently been made by Soddy that the products of radioactive disintegration appear to pass in a symmetrical way through positions in the periodic system, giving off a helium molecule at alternate transformations until the place of lead is reached. It appears, therefore, that the five vacant places in the table above bismuth are probably occupied by these evanescent elements, and it is to be noticed that all of the elements that have been placed in this region of high atomic weights are radioactive.

There are some inconsistencies in the periodic system. The increments in the atomic weights are irregular, and there are three cases, argon and potassium, cobalt and nickel, and tellurium and iodine, where a higher atomic weight is placed before a lower one in order to bring these elements into their undoubtedly proper places. There is a peculiarity also in the heavy-metal division of Group VIII, where three similar elements occur in each of three places, and where the usual periodicity appears to be suspended, or nearly so, in comparison with most of the other elements. However, there seems to be a still more remarkable case of this kind in Group III, where fourteen metals of the rare-earths have been placed. They are astonishingly similar in their chemical properties, hence it seems necessary to assume that periodicity is suspended here throughout the wide range of atomic weights from 139 to 174, where no elements save these have been found.

Several other interesting features of the table may be pointed out. The chlorides and hydrides, as indicated by the "typical compounds," show a regular progression in both directions towards Group IV. (Where the type-formulas do not apply, as far as is known, to more than one or two elements, they have been placed in parentheses in the table given here.) It is a striking fact that the acid-forming elements occur together in a definite part of the table, and that the gases and other non-metallic elements, except the inactive gases of Group VIII, occur in the same region.

Atomic Numbers.—As the result of a spectroscopic study of the wave-lengths or frequencies of the X-rays produced when cathode rays strike upon anti-cathodes composed of different elements, Moseley in 1914 discovered that whole numbers in a simple series can be attributed to the atoms. These atomic numbers are: 1 for hydrogen, 2 for helium, 3 for lithium, 4 for beryllium, and so on, in the order in which the elements occur in Mendeléeff's periodic table, and in the cases of argon and potassium, cobalt and nickel, and tellurium and iodine, they follow the correct chemical order, while the atomic weights do not. They appear to indicate, therefore, an even more fundamental relation between the atoms than that shown by the atomic weights.

These numbers are now available for every element up to lead, and they are particularly interesting in indicating, on account of missing numbers, the existence of two undiscovered elements in the manganese group, and two more among the rare-earth metals, in confirmation of the vacant places below lead in Mendeléeff's table.

The Isolation of Elements.—In the year 1818 about 53 elements were recognized, and since that time about 30 more have been discovered, but the elements already known comprised the more common ones, and nearly all of those which have been commercially important. A few of them, including beryllium, aluminium, silicon, magnesium, and fluorine, were then known only in their compounds, as they had not yet been isolated in the free condition.

Berzelius in 1823 prepared silicon, a non-metallic element resembling carbon in many respects. This element has recently been prepared on a rather large scale in electric furnaces at Niagara Falls, and has been used for certain purposes in the form of castings.

Wöhler created much sensation in 1827 by isolating aluminium and finding it to be a very light, strong and malleable metal, stable in the air, and of a silver-white color. For a long time this metal was a comparative rarity, being prepared by the reduction of aluminium chloride with metallic sodium; but about 25 years ago Hall, an American, devised a method of preparing it by

electrolyzing aluminium oxide dissolved in fused cryolite. This process reduced the cost of aluminium to such an extent that it has now come into common use.

Wöhler and Bussy prepared beryllium in 1828, and Liebig and Bussy did the same service for magnesium in 1830. The latter metal has come to be of much practical importance, both as a very powerful reducing agent in chemical operations, and as an ingredient of flash-light powders and of mixtures used for fireworks. It is also used in making certain light alloys.

After almost innumerable attempts to isolate fluorine, during a period of nearly a century, this was finally accomplished in 1886 by Moissan in France by the electrolysis of anhydrous hydrogen fluoride. The free fluorine proved to be a gas of extraordinary chemical activity, decomposing water at once with the formation of hydrogen fluoride and ozonized oxygen. This fact explains the failure of many previous attempts to prepare it in the presence of water.

Early Discoveries of New Elements.—The remarkable activity of chemical research at the beginning of our period is illustrated by the fact that three new elements were discovered in 1817. In that year Berzelius had discovered selenium, Arfvedson, working in Berzelius's laboratory had discovered the important alkali-metal lithium, and Stromeyer had discovered cadmium.

In 1826 Ballard in France discovered bromine in the mother-liquor from the crystallization of common salt from sea-water. Bromine proved to be an unusually interesting element, being the only non-metallic one that is liquid at ordinary temperatures, and being strikingly intermediate in its properties between chlorine and iodine. It has been obtained in large quantities from brines, and is produced extensively in the United States. The elementary substance and its compounds have found important applications in chemical operations, while the bromides have been found valuable in medicine and silver bromide is very extensively used in photography.

In 1828 Berzelius discovered thorium. The oxide of this metal has recently been employed extensively as the principal constituent of incandescent gas-mantles, and the element has acquired particular importance from the

fact that, like uranium, it is radio-active, decomposing spontaneously into other elements.

Vanadium had been encountered as early as 1801 by Del Rio, who named it "erythronium," but a little later it was thought to be identical with chromium and was lost sight of for a while. In 1830, however, it was re-discovered by, and received its present name from Sefström in Sweden. Berzelius immediately made an extensive study of vanadium compounds, but he gave them incorrect formulas and derived an incorrect atomic weight for the element, because he mistook a lower oxide for the element itself. Roscoe in England in 1867 isolated vanadium for the first time, found the right atomic weight, and gave correct formulas to its compounds. Vanadium is particularly interesting from the fact that it displays several valencies in its compounds, many of which are highly colored. It has found important use as an ingredient in very small proportions in certain "special steels" to which it imparts a high degree of resistance to rupture by repeated shocks.

Columbium was discovered early in the nineteenth century in the mineral columbite from Connecticut by Hatchett, an Englishman, who did not, however, obtain the pure oxide. It was afterwards obtained by Rose who named it niobium. Both names for the element are in use, but the former has priority. Attention was called to this fact by an article in the Journal by Connell, an Englishman (**18**, 392, 1854).

The Platinum Group of Metals.—In 1854 a new member of the platinum group of metals, ruthenium, was discovered by Claus. Platinum had been discovered about the middle of the eighteenth century, while its other rarer associates, iridium, osmium, palladium, and rhodium, had been recognized in the very early years of the nineteenth century. It was during the latter period that platinum ware began to be employed to a considerable extent in chemical operations, and this use was greatly extended as time went on. The discovery was made by Phillips in 1831 that finely divided platinum by contact would bring about the combination of sulphur dioxide with atmospheric oxygen, and this application during the past 20 years has become enormously important in the sul-

phuric acid industry, while other important applications of platinum as a "catalytic agent" have also been made. Wolcott Gibbs and Carey Lea have contributed perhaps more than any other recent chemists to a knowledge of the platinum metals. Carey Lea (**38**, 81, 248, 1864) dealt chiefly with the separation of the metals from each other, while Gibbs's work (**31**, 63, 1861; **34**, 341, 1862) included investigations of many of the compounds.

It may be mentioned that while platinum and its associates were formerly known only in the uncombined condition in nature, the arsenide sperrylite, $PtAs_2$, was described by the late S. L. Penfield, and the senior writer of this chapter, in articles published in the Journal (**37**, 67, 71, 1889).

Applications of the Spectroscope.—The discovery in certain mineral waters of the rare alkali-metals rubidium and cæsium by Bunsen and Kirchoff in 1861 was in consequence of the application of spectroscopy by these same scientists a short time previously to the identification of elements imparting colors to the flame. Since that time the employment of the spectroscope for chemical purposes has been much extended, as it has been used in the examination of light from electric sparks and arcs, as well as from Geissler tube discharges and from colored solutions.

The metals rubidium and cæsium are interesting in being closely analogous to potassium and in standing at the extreme electro-positive end of the series of known metals. It should be noticed here that Johnson and Allen of our Sheffield Laboratory, having obtained a good supply of rubidium and cæsium material from the lepidolite of Hebron, Maine, made some important researches upon these elements, accounts of which were published in the Journal (**34**, 367, 1862; **35**, 94, 1863). They established the atomic weight of cæsium, thus correcting Bunsen's determination which was unsatisfactory on account of the small quantity and impurity of his material. Pollucite, a mineral rich in cæsium, which had been found in very small amount on the Island of Elba, has more recently been obtained in large quantities—hundreds of pounds—at Paris, Maine, and its vicinity. This American pollucite was first analyzed and identi-

fied by the senior writer of this article (**41**, 213, 1891), and later (**43**, 17, 1892 *et seq.*) the results of many investigations on cæsium and rubidium compounds, in which the junior writer played an important part, carried out in Sheffield Laboratory, were published in the Journal.

The application of the spectroscope led to the discovery of thallium in 1861 by Crookes of England, and to that of indium in 1863 by Reich and Richter in Germany. Both of these metals are extremely rare, but they are of considerable theoretical interest. Thallium is particularly remarkable in showing resemblances in its different compounds to several groups of metals.

The spectroscope was employed again in connection with the discovery of gallium in 1875 by Boisbaudran. It is in the same periodic group as thallium and indium, and it has a remarkably low melting point, just above ordinary room-temperature. It has been among the rarest of the rare elements, but within two or three years a source of it has been found in the United States in certain residues from the refining of commercial zinc. The recent issues of the Journal (**41**, 351, 1916; **42**, 389, 1916) show that Browning and Uhler of Yale have availed themselves of this new material in order to make important chemical and physical researches upon this metal.

Germanium.—The discovery of germanium in the mineral argyrodite in 1886 by Winkler revealed a curious metal which gives a white sulphide that may be easily mistaken for sulphur and which is volatilized completely when its hydrochloric acid solution is evaporated, so that it is evasive in analytical operations. This element had been predicted with much accuracy by Mendeléeff, and it is rather closely related to tin.

A few years after the discovery of germanium, Penfield published in the Journal (**46**, 107, 1893; **47**, 451, 1894) some analyses of argyrodite, correcting the formula given by Winkler to the mineral; also he described canfieldite, an analogous mineral from Bolivia, in which a large part of the germanium was replaced by tin.

The Rare Earths.—Before the year 1818 two rare earths, the oxides of yttrium and cerium, were known in an impure condition. Since that time about fourteen others have been discovered as associates of the first

two. The rare earths are peculiar from the fact that many of them are always found mixed together in the minerals containing them, and also from the circumstance that most of them are remarkably similar in their chemical reactions and consequently exceedingly difficult to separate from each other. In many cases multitudes of fractional precipitations or crystallizations are needed to obtain pure salts of a number of these metals. The solutions of the salts of several of these elements give characteristic absorption bands when examined spectroscopically by the use of transmitted light.

No important practical application has been found for any of these earthy oxides, except that about one per cent of cerium oxide is mixed with thorium oxide in incandescent gas-mantles in order to obtain greatly increased luminosity.

The Inactive Gases.—As long ago as 1785, Cavendish, that remarkable Englishman who first weighed the world and first discovered the composition of water, actually obtained a little argon in a pure condition by sparking atmospheric nitrogen with oxygen converting it into nitric acid (another discovery of his) and absorbing the excess of oxygen. The volume of this residual gas as estimated by him corresponds very closely to the volume of argon in the atmosphere, as now known.

It was more than a century later, in 1894, that Rayleigh and Ramsay discovered argon in the air. Lord Rayleigh had found that atmospheric nitrogen was about one-half per cent heavier than chemical nitrogen, a fact which led to the investigation. It was only necessary to repeat Cavendish's experiment on a large scale, or to absorb oxygen with hot copper and nitrogen with hot magnesium, in order to obtain argon. The gas attracted much attention, both on account of having but a single atom in its molecule, and particularly because it failed to enter into chemical combination of any kind. This gas has been used of late for filling the bulbs of incandescent electric lamps in cases where a gas-pressure without chemical action is desired.

In 1890 and 1891, Hillebrand published in the Journal **40**, 384, 1890: **42**, 390, 1891) a series of analyses of the mineral uraninite and reported in some samples of the

mineral as much as 2·5 per cent of an inactive gas. Hillebrand examined the gas spectroscopically but, just missing an important discovery, he detected only the spectrum lines of nitrogen. Ramsay, in searching for argon in some sort of natural combination, and doubtless remembering Hillebrand's work, heated some cleveite, a variety of uraninite, and obtained, not argon, but a new gas. This gave a yellow spectrum-line corresponding to a line previously observed in the light of the sun's corona and attributed to an element in the sun called helium. Helium, therefore, in 1895 had been found on the earth. This gas is a constant constituent of uranium minerals, as it is produced by the breaking down of radioactive elements. It has been found in very small quantity in the atmosphere, and is the most difficult of all known gases to liquefy, as its boiling point, as shown by Onnes in 1908, is only 4° above the absolute zero. It has not yet been solidified.

In 1898 Ramsay and Travers, by the use of ingenious methods of fractional distillation and absorption by charcoal, obtained three other much rarer inactive gases from the atmosphere which they called neon, krypton and xenon.

The inactive gases are all colorless, and as they form no chemical compounds they are characterized by their densities, which give their atomic weights, by their boiling points, and by their characteristic Geissler-tube spectra.

The gaseous radium emanation, or niton, belongs also to the inactive group, and it was also collected and studied by Ramsay who was compelled to work with only 0·0001 cc. of it, as the volume obtained by heating radium salts is very small. It is an evanescent element, disappearing within a few days on account of radioactive disintegration. Meanwhile it glows brilliantly when liquefied and cooled to the temperature of liquid air. It has an atomic weight of 222, four units below that of radium, and the difference is considered as due to the loss by radium of an atom of helium in passing into the emanation.

The Radioactive Elements.—The discovery of radium in 1898 by Madame Curie, and the study of that and other

radioactive elements has produced a profound effect upon chemical theory. It was found that the two elements of the highest atomic weights, uranium and thorium, are always spontaneously decomposing into other elements at a fixed rate of speed which can be controlled by no artificial means, and that the elements resulting from these decompositions likewise undergo spontaneous changes into still other elements at greatly varying rates of speed, forming in each case a remarkable series of temporary elements. These transformations are accompanied by the emission at enormous velocities of three kinds of rays, one variety of which has been shown to consist of helium atoms. The greater number of the elements formed in these transformations have not as yet been obtained in a pure condition, and they are known only in connection with their radioactivity, volatility, etc.; but radium and niton, two of these products, have been obtained in a pure condition, so that their atomic weights and their places in the periodic system have been fixed.

We owe much of our knowledge of the radioactive transformations to the researches of Rutherford and of Soddy, and of their co-workers, but one of the important products of the transformation of uranium, an element which he called ionium, was characterized by Boltwood of Yale (**25**, 365, 1908).

Radium and niton, apart from their radioactive properties, resemble barium and the inert gases of the atmosphere, respectively. The rates at which their progenitors produce them, and the rates at which they themselves decompose, bring about a state of equilibrium after a time. Therefore a given amount of uranium, which decomposes exceedingly slowly, can yield even after thousands of years only a very small proportional quantity of undecomposed radium, one-half of which disappears in about 2500 years, because the amount decomposed must eventually be equal to the amount produced. The first conclusive evidence that radium is a product of the decomposition of uranium was given by Boltwood in the Journal (**18**, 97, 1904). He found that all uranium minerals contain radium; and the amount of radium present is always proportional to the amount

of uranium, which shows the genetic relation between the two.

In the case of niton, which is produced by radium, and is called also the radium emanation, the rate of decay is rapid, so that if the gas is expelled from radium by heating, equilibrium is reached after a few days, with the accumulation of the largest possible amount of niton.

The conclusion has been reached by Rutherford and others that the final product besides helium, in the radioactive transformations, is lead, or at least an element or elements resembling lead to such a degree that no separation of them by chemical means is possible. Atomic weight determinations by Richards and others have shown that specimens of lead found in radioactive minerals give distinctly different atomic weights from that of ordinary lead. This fact has led to the view that possibly the atoms of the elements are not all of the same weight, but vary within certain limits—a view that is contrary to previous conclusions derived from the uniformity in atomic weights obtained with material from many different sources.

The results of the investigations upon radioactivity have led to modified views in regard to the stability of the elements in general. There has been little or no proof obtained that any artificial transmutation of the elements is possible, but the spontaneous transformation of the radioactive elements brings forward the possibility that other elements are changing imperceptibly, and that a state of evolution exists among them. All of the radioactive changes that we know proceed from higher to lower atomic weights, and we are entirely ignorant of the process by which uranium and thorium must have been produced originally.

Since radioactive changes have been found to be accompanied by the release of vast amounts of energy, compared with which the energy of chemical reactions is trivial, a new aspect in regard to the structure of atoms has arisen,—they must be complex in structure, the seats of enormous energy.

The determination of the amount of radium in the earth's crust has indicated that the heat produced by it is amply sufficient to supply the loss of heat due to radia-

tion, and this source of heat is regarded by many as the cause of volcanic action. The sun's radiant heat also has been supposed to be supplied by radioactive action, so that the older views regarding the limitation of the age of the earth and the solar system on account of loss of heat have been considerably modified by our knowledge of radioactivity.

Physical Chemistry.

The application of physical methods as aids to chemical science began in early times, and some of these, such as the determinations of gas and vapor densities, specific heats, and crystalline forms have been mentioned already in this article. Within recent times physical chemistry has greatly developed and a few of its important achievements will now be described.

Molecular Weight Determinations.—Gas and vapor densities in connection with Avogadro's principle, formed the only basis for molecular weight determinations until comparatively recent times. The early methods of Gay-Lussac and Dumas for vapor density were supplemented in 1868 by the method of Hofmann, whereby vapors were measured under diminished pressure over mercury. In 1878 Victor Meyer introduced a simpler method depending upon the displacement of air or other gas by the vapor in a heated tube. As refractory tubes, such as those of porcelain or even iridium, could be used in this method, molecular weights at extremely high temperatures were determined with interesting results. For instance, it was found that iodine vapor, which shows the molecule I_2 at lower temperatures, gradually becomes monatomic with rise in temperature, that sulphur vapor dissociates from S_8 to S_2 under similar conditions, and that most of the metals, including silver, have monatomic vapors.

In 1883 and later it was pointed out by Raoult that the molecular weights of substances could be found from the freezing points of their solutions, but this method was complicated from the fact that salts, strong acids and strong bases behaved quite differently from other substances in this respect, and allowances had to be made for the types of substances used. The complication was

afterwards explained by the ionization theory of Arrhenius. Better apparatus for this method was soon devised by Beckmann, who introduced also a method depending upon the boiling points of solutions, and these two methods are still the standard ones for determining molecular weights in solution. They are very extensively employed by organic chemists.

It has been found that the majority of substances when dissolved have the same molecular weight as in the gaseous condition, provided that they can be volatilized at comparable temperatures. For instance, sulphur in solution has the formula S_8, iodine is I_2 and the metals are monatomic.

Van't Hoff's Law and Arrhenius's Theory of Ions.—Modern views on solutions date largely from 1886, when van't Hoff called attention to the relations existing between the osmotic pressure exerted by dissolved substances and gas pressure.

Pfeffer, a botanist, was the first to measure osmotic pressure (1877). Basing his conclusions chiefly upon Pfeffer's determinations, van't Hoff formulated a new and highly important law, which may be stated as follows: The osmotic pressure exerted by a substance in solution is equal to the gas pressure that the substance would exert if it were a gas at the same temperature and the same volume. Further investigations have fully established the fact that molecules in dilute solution obey the simple laws of gases.

It was pointed out by van't Hoff that salts, strong acids and strong bases showed marked exceptions to his law in exerting much greater osmotic pressures than those calculated for them.

The next year in 1887, Arrhenius explained this abnormal behavior of salts, strong acids and strong bases by assuming that they dissociate spontaneously into ions when they dissolve, and that these more numerous particles act like molecules in producing osmotic pressure. He showed that these exceptional substances all conduct electricity in solution, while those conforming with van't Hoff's law do not, and according to his theory the ions become positively or negatively charged when they are formed, and these charged ions conduct the current.

For example a molecule of sodium chloride was supposed to give the two ions Na^+ and Cl^-, thus exerting twice as much osmotic pressure as a single molecule.

Determinations of osmotic pressure or related values, such as depression of the freezing point and of electric conductivity, indicated that ionization could not be regarded as complete in any case except in exceedingly dilute solutions, and that the extent of ionization varied with different substances. The fact that osmotic pressures and electric conductivities gave closely agreeing results in regard to the extent of ionization in various cases, is the strongest evidence in support of the theory.

It was difficult at first for many chemists to believe that atoms, such as those of sodium and chlorine, and groups such as NH_4 and SO_4 could exist independently in solution, even though electrically charged. However, the theory rapidly gained ground and is now accepted by nearly every chemist as a satisfactory explanation of many facts.

During recent years, many investigations relating to osmotic pressure and ionization have been carried out in the United States, but only the work of Morse, A. A. Noyes, and the late H. C. Jones can be merely alluded to here. It should be mentioned that the eminent author of the ionic hypothesis gave the Silliman Memorial course of lectures at Yale in 1911 on Theories of Solution.

Colloidal Solutions.—Graham, an English chemist, in 1861 was the first to make a distinction between substances forming true solutions, which he called crystalloids, and those of a gummy nature resembling glue, which in solution do not diffuse readily through parchment membranes, as crystalloids do, and which he called colloids. The separation of colloids by means of parchment was called dialysis, and this process has come into extensive use in preparing pure colloidal solutions. Slow diffusion is now regarded as characteristic of colloids rather than their gummy condition.

Colloidal solutions occupy an intermediate position between true solutions and suspensions, resembling one or the other according to the kind of colloid and the fineness of division. By preparing filters with pores of varying degrees of fineness, Bechold has been able to

separate colloids from each other in accordance with the size of their particles. It has also been possible to prepare different solutions of a colloid varying gradually from one in which the particles were undoubtedly in suspension to one which had many of the properties of a true solution.

Beginning in 1889, Carey Lea described in the Journal **37**, 476, 1889 *et seq.*) a variety of methods for preparing colloidal solutions of the metals, consisting in general of treating solutions of metallic salts with mild reducing agents. His work on colloidal silver was particularly extensive and interesting. Solutions of this kind have recently yielded some extremely interesting results by means of the ultra-microscope, an apparatus devised by Zsigmondy and Siedentopf. A very intense beam of light is passed through the solution and observed at right angles with a powerful microscope. Under these conditions, particles much too small to be seen by other means, reveal their presence by reflected light. It has been possible in a very dilute solution of known strength to count the particles and thus to calculate their size. The smallest colloidal particles measured in this way were of gold and were shown to have approximately ten times the diameter, or 1000 times the volume, attributed to ordinary molecules. It is of interest that the particles appear in rapid motion corresponding to the well-known Brownian movement.

The chemistry of colloids has now assumed such importance that it may be considered as a separate branch of the science. It has its own technical journal and deals largely with the chemistry of organic products. All living matter is built up of colloids, and hæmoglobin, starch, proteins, rubber and milk are examples of colloidal substances or solutions. Among inorganic substances, many sulphides, silicic acid, and the amorphous hydroxides, like ferric hydroxide, frequently act as colloids.

Law of Mass-Action.—Berthollet about the beginning of the last century was the first chemist to study the effect of mass, or more correctly, the concentration of substances on chemical action. His views summarized by himself are as follows: "The chemical activity of a

substance depends upon the force of its affinity and upon the mass which is present in a given volume.'' The development of this idea, which is fundamentally correct, was greatly hindered by the fact that Berthollet drew the incorrect conclusion that the composition of chemical compounds depended upon the masses of the substances combining to produce them, a conclusion in direct contradiction to the law of definite proportions, and since this view was soon disproved by Proust and others, Berthollet's law in its other applications received no immediate attention. Mitchell, however, pointed out in the Journal (**16**, 234, 1829) the importance of Berthollet's work, and Heinrich Rose in 1842 again called attention to the effect of mass, mentioning as one illustration the effect of water and carbonic acid in decomposing the very stable natural silicates. Somewhat later several other chemists made important contributions to the question of the influence of concentration upon chemical action, but it was the Norwegians, Guldberg and Waage, who first formulated the law of mass action in 1867.

This law has been of enormous importance in chemical theory, since it explains a great many facts upon a mathematical basis. It applies particularly to equilibrium in reversible reactions, where it states that the product of the concentrations on the one side of a simple reversible equation bears a constant relation to the products of the concentrations on the other side, provided that the temperature remains constant. In cases of this kind where two gases or vapors react with two solids, the latter if always in excess may be regarded as constant in concentration, and the law takes on a simpler aspect in applying only to the concentrations of the gaseous substances. For example, in the reversible reaction

$$3Fe + 4H_2O \rightleftarrows Fe_3O_4 + 4H_2,$$

which takes place at rather high temperatures, a definite mixture of steam and hydrogen at a definite temperature will cause the reaction to proceed with equal rapidity in both directions, thus maintaining a state of equilibrium, provided that both iron and the oxide are present in

excess. If, however, the relative concentrations of the hydrogen and steam are changed, or even if the temperature is changed, the reaction will proceed faster in one direction than in the other until equilibrium is again attained.

The principle of mass-action also explains why it is sometimes possible for a reversible reaction to become complete in either direction. For instance, in connection with the reaction that has just been considered, if steam is passed over heated iron and if hydrogen is passed over the heated oxide, the gaseous product in each case is gradually carried away, and the reaction continually proceeds faster in one direction than in the other until it is complete, according to the equations

$$3Fe + 4H_2O \longrightarrow 3Fe_3O_4 + 4H_2 \quad \text{and}$$
$$Fe_3O_4 + 4H_2 \longrightarrow 3Fe + 4H_2O.$$

Many other well-known and important facts, both chemical and physical, depend upon this law. It explains the circumstance that a vapor-pressure is not dependent upon the amount of the liquid that is present; it also explains the constant dissociation pressure of calcium carbonate at a given temperature, irrespective of the amounts of carbonate and oxide present; in connection with the ionic theory, it furnishes the reason for the variable solubility of salts due to the presence of electrolytes containing ions in common; and it elucidates Henry's law which states that the solubilities of gases are proportional to their pressures.

Ostwald, more than any other chemist, has been instrumental in making general applications of this law, and he made particularly extensive use of it in connection with analytical chemistry in a book upon this subject which he published.

The Phase Rule.—In 1876 Willard Gibbs of Yale published a paper in the Proceedings of the Connecticut Academy of Science on the "Equilibrium of Heterogeneous Substances," and two years later he published an abstract of the article in the Journal (**16**, 441, 1878). He had discovered a new law of nature of momentous importance and wide application which is called the

"Phase-Rule" and is expressed by a very simple formula.

The application of this great discovery to chemical theory was delayed for ten years, partly, perhaps, because it was not sufficiently brought to the attention of chemists, but largely it appears because it was not at first understood, since its presentation was entirely mathematical.

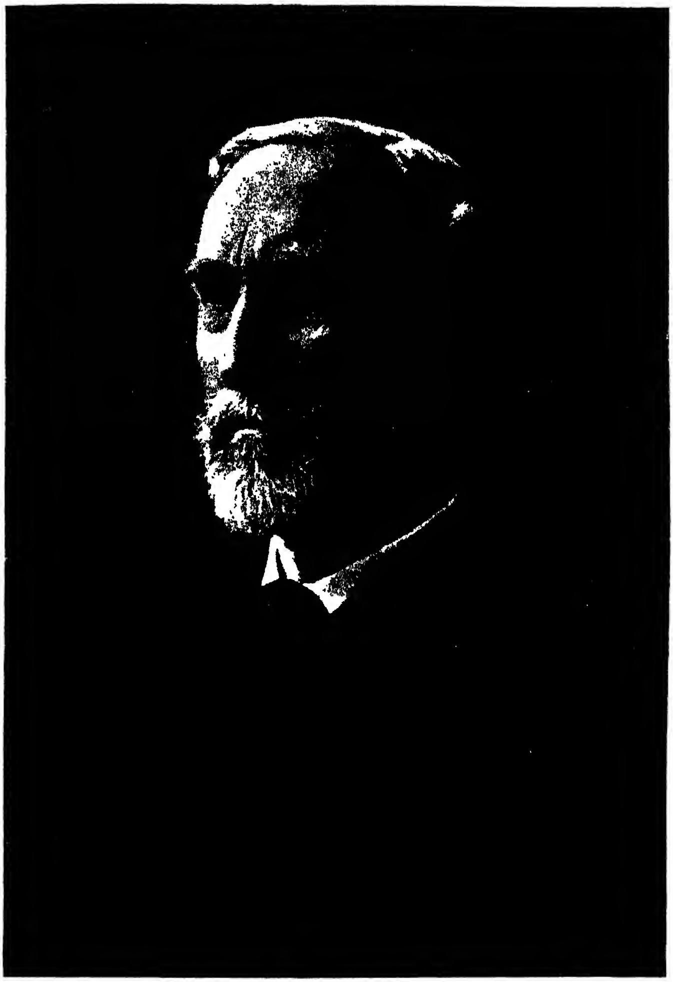
It was Rooseboom, a Dutch chemist, who first applied the phase-rule. It soon attracted profound attention, and the name of Willard Gibbs attained world-wide fame among chemists. When Nernst, who is perhaps the most eminent physical chemist of the present time, was delivering the Silliman Memorial Lectures at Yale a few years ago, he took occasion to place a wreath on the grave of Willard Gibbs in recognition of his achievements.

To understand the rule, it is necessary to define the three terms, introduced by Gibbs, *phase, degrees of freedom* and *component.*

By the first term, is meant the parts of any system of substances which are mechanically separable. For instance, water in contact with its vapor has two phases, while a solution of salt and water is composed of but one. The degrees of freedom are the number of physical conditions, including pressure, temperature and concentration, which can be varied independently in a system without destroying a phase. The exact definition of a component is not so simple, but in general, the components of a system are the integral parts of which it is composed. Any system made up of the compound H_2O, for instance, whether as ice, water or vapor, contains but one component, while a solution of salt and water contains two. Letting P, F. and C stand for the three terms, the phase-rule is simply

$$F = C + 2 - P$$

that is, the number of degrees of freedom in a system in equilibrium equals the number of components, plus two, minus the number of phases. The rule can be easily understood by means of a simple illustration. In a system composed of ice, water and water-vapor, there are three phases and one component and therefore



J. Willard Gibbs

$$F = 1 + 2 - 3 = 0$$

Such a system has no degrees of freedom. This means that no physical condition, pressure or temperature can be varied without destroying a phase, so that such a system can only exist in equilibrium at one fixed temperature, with a fixed value for its vapor-pressure.

For instance, if the system is heated above the fixed temperature, ice disappears and if the pressure is raised, vapor is condensed. If this same system of water alone contains but two phases, for instance, liquid and vapor, $F = 1 + 2 - 2 = 1$, or there is one degree of freedom. In such a system, one physical condition such as temperature can be varied independently, but only one, without destroying a phase. For instance, the temperature may be raised or lowered, but for every value of temperature there is a corresponding value for the vapor pressure. One is a function of the other. If both values are varied independently, one phase will disappear, either vapor condensing entirely to water or the reverse. Finally if the system consists of one phase only, as water vapor, $F = 2$, or the system is divariant, which means that at any given temperature it is possible for vapor to exist at varying pressures.

The illustration which has been given relates to physical equilibrium, but the rule is applicable to cases involving chemical changes as well. In comparing the phase-rule with the law of mass action, it will be noticed that both have to do with equilibrium. The great advantage of the former is that it is entirely independent of the molecular condition of the substances in the different phases. For instance, it makes no difference so far as the application of the rule is concerned, whether a substance in solution is dissociated, undissociated or combined with the solvent. In any case, the solution constitutes one phase. On the other hand, the rule is purely qualitative, giving information only as to whether a given change in conditions is possible. The law of mass action is a quantitative expression so that when the value of the constant is once known, the change can be calculated which takes place in the entire system if the concentration of one substance is varied. The law, however, requires a knowledge of the molecular condition of

the reacting substances, which may be uncertain or unknown, and chiefly on this account it has, like the phase-rule, often only a qualitative significance.

The phase rule has served as a most valuable means of classifying systems in equilibrium and as a guide in determining the possible conditions under which such systems can exist. As illustrations of its practical application, van't Hoff used it as an underlying principle in his investigations on the conditions under which salt deposits have been formed in nature, and Rooseboom was able by its means to explain the very complicated relations existing in the alloys of iron and carbon which form the various grades of wrought iron, steel and cast iron.

Thermochemistry.—This branch of chemistry has to do with heat evolved or absorbed in chemical reactions. It is important chiefly because in many cases it furnishes the only measure we have of the energy changes involved in reactions. To a great extent, it dates from the discovery by Hess in 1840 of a fundamental law which states that the heat evolved in a reaction is the same whether it takes place in one or in several stages. This law has made it possible to calculate the heat values of a large number of reactions which cannot be determined by direct experiment.

Thermochemistry has been developed by a comparatively few men who have contributed a surprisingly large number of results. Favre and Silbermann, beginning shortly after 1850, improved the apparatus for calorimetric determinations, which is called the calorimeter, and published many results. At about the same time Julius Thomsen, and in 1873 Berthelot, began their remarkable series of publications which continued until recently. Thomsen's investigations were published in 1882 in 4 volumes. It is probably safe to say that the greater part of the data of thermochemistry was obtained by these two investigators. The bomb calorimeter, an apparatus for determining heat values by direct combustion, was developed by Berthelot. The recent work of Mixter at Yale, published in the Journal, and of Richards at Harvard should be mentioned particularly. Mixter's work in this field began in 1901 (**12**, 347). Using an improved bomb calorimeter, he has developed a

method of determining the heats of formation of oxides by combustion with sodium peroxide. By this same method as well as by direct combustion in oxygen, he has obtained results which appear to equal or excel in accuracy any which have ever been obtained in his field of work. Richards's work has consisted largely of improvements in apparatus. He developed the so-called adiabatic calorimeter which practically eliminates one of the chief errors in thermal work caused by the heating or cooling effect of the surroundings. This modification is being generally adopted where extremely accurate work is required.

Organic Chemistry.

One hundred years ago qualitative tests for a few organic compounds were known, the elements usually occurring in them were recognized, and some of them had been analyzed quantitatively, but organic chemistry was far less advanced than inorganic, and almost the whole of its enormous development has taken place during our period.

Berzelius made a great advance in the subject by establishing the fact, which had been doubted previously, that the elements in organic compounds are combined in constant, definite proportions. In 1823 Liebig brought to light the exceedingly important fact of isomerism by showing that silver fulminate had the same percentage composition as silver cyanate, a compound of very different properties. Isomeric compounds with identical molecular weight as well as the same composition have since been found in very many cases, and they have played a most important part in determining the arrangements of atoms in molecules. They have been found to be very numerous in many cases. For instance, three pentanes with the formula C_5H_{12} are known, all that are possible according to theory, and in each case the structure of the molecule has been established. On theoretical grounds it has been calculated that 802 isomeric compounds with the formula $C_{13}H_{28}$ are possible, while with more complex formulas the numbers of isomers may be very much greater.

A particularly interesting case of isomerism was observed by Wöhler in 1828, when he found that ammonium cyanate changes spontaneously into urea

$$(NH_4CNO \longrightarrow N_2H_4CO).$$

This was the first synthesis of an organic compound from inorganic material, and it overthrew the prevailing view that vital forces were essential in the formation of organic substances. A great many natural organic compounds have been made artificially since that time, and some of them, such as artificial alizarin, indigo, oil of wintergreen, and vanillin, have more or less fully replaced the natural products. The preparation of a vast number of compounds not known in nature, many of which are of practical importance as medicines, dyes, explosives, etc., has been another great achievement of organic chemistry.

The development of our present formulas for organic compounds, by means of which in many cases the relative positions of the atoms can be shown with the greatest confidence, has been gradual. Formulas based on the dualistic idea of Berzelius were used for some time, type formulas, with the employment of compound radicals, came later, the substitution of atoms or groups of atoms for others in chemical reactions came to be recognized, but one of the most important steps was the recognition of the quadrivalence of carbon and the general application of valency to atoms by Kekulé about 1858. This led directly to the use of modern structural formulas which have been of the greatest value in the theoretical interpretation of organic reactions. It was Kekulé also who proposed the hexagonal ring-formula for benzene, C_6H_6, which led to exceedingly important theoretical and practical developments. The details of the formulas for many other rings and complex structures have been established since that time, and there is no doubt that the remarkable achievements in organic chemistry during the past sixty years have been much facilitated by the use of these formulas.

Many important researches in organic chemistry have been carried out in the United States, and the activity in this direction has greatly increased in recent years. In

this connection the large amount of work of this kind accomplished in the Sheffield Laboratory, at present under the guidance of Professor T. B. Johnson, should be mentioned.

It has happened that comparatively few publications on organic chemistry have appeared in the Journal, but it may be stated that the preparation of chloroform and its physiological effects were described by Guthrie (**21**, 64, 1832). Unknown to him, it had been prepared by Souberain, a French chemist, the previous year, but the former was the first to describe its physiological action. Silliman gave a sample to Doctor Eli Ives of the Yale Medical School, who used it to relieve a case of asthma. This was the first use of chloroform in medical practice (**21**, 405, 1832). Guthrie also described in the Journal (**21**, 284, 1832) his new process for converting potato starch into glucose, a method which is essentially the same as that used to-day in converting cornstarch into glucose. Lawrence Smith (**43**, 301, 1842 *et seq.*), Horsford (**3**, 369, 1847 *et seq.*), Sterry Hunt (**7**, 399, 1849), Carey Lea (**26**, 379, 1858 *et seq.*), Remsen (**5**, 179, 1873 *et seq.*), and others have contributed articles on organic chemistry.

Agricultural Chemistry.

Until near the middle of the nineteenth century, it was believed that plants, like animals, used organic matter for food, and depended chiefly upon the humus of the soil for their growth. This view was held even long after it was known that plant leaves absorb carbon dioxide and give off oxygen, and after the ashes of plants had been accurately analyzed.

This incorrect view was overthrown by the celebrated German chemist, Liebig, who made many investigations upon the subject, and, properly interpreting previous knowledge, published a book in 1840 upon the application of chemistry to agriculture and physiology in which he maintained that the nutritive materials of all green plants are inorganic substances, namely, carbon dioxide, water, ammonia (nitrates), sulphates, phosphates, silica, lime, magnesia, potash, iron, and sometimes common salt. He drew the vastly important conclusion that the effective

fertilization of soils depends upon replenishing the inorganic substances that have been exhausted by the crops.

The fundamental principles set forth by Liebig have been confirmed, and it has been found that the fertilizing constituents most commonly lacking in soils are nitrogen compounds, phosphates, and potassium salts, so that these have formed the important constituents of artificial fertilizers. Liebig himself found that humus is valuable in soils, because it absorbs and retains the soluble salts.

The foundation established by Liebig in regard to artificial fertilizers has led to an enormous application of these materials, much to the advantage of the world's food-supply.

It was Liebig's belief, in accordance with the prevailing views, that decay and putrefaction as well as alcoholic and other fermentations were spontaneous processes, and when the eminent French chemist, Pasteur, in 1857, explained fermentation as directly caused by yeast, an epoch-making discovery which led to the explanation of decay and putrefaction by bacterial action and to the germ-theory of disease, the explanation was violently opposed by Liebig and other German chemists. Pasteur's view prevailed, however, and since that time it has been found that various kinds of bacteria are responsible for the formation of ammonia from nitrogenous organic matter and also for the change of ammonia into the nitrates that are available as plant-food.

The long-debated question as to the availability of atmospheric nitrogen for plant-food was settled in 1886 by the discovery of Hellriegel that bacteria contained in nodules on the roots, especially of leguminous plants, are capable of bringing nitrogen into combination and furnishing it to the plants.

No more than an allusion can be made to agricultural experiment stations where soils, fertilizers, foods and other products are examined, and where other problems connected with agriculture are studied.

The late S. W. Johnson of Yale studied with Liebig and subsequently did much service for agricultural chemistry in this country, by his investigations, his teaching, and his writings. His book, "How Crops Grow," pub-

lished in 1868, gave an excellent account of the principles of agricultural chemistry. He did much to bring about the establishment of agricultural experiment stations in this country, and for a long time he was the director of the Connecticut Station.

In the Journal, as early as 1827, Amos Eaton (**12**, 370) published a simple method for the mechanical analysis of soils to determine their suitability for wheat-culture, and Hilgard, between 1872 and 1874, described an elaborate study of soil-analysis. J. P. Norton, a Yale professor, in 1847 (**3**, 322) published an investigation on the analysis of the oat, which was awarded a prize of fifty sovereigns by a Scotch agricultural society, while Johnson, Atwater, and others have contributed articles on the analysis of various farm products.

Industrial Acids and Alkalies.

One hundred years ago sulphuric acid was manufactured on a comparatively very small scale in lead chambers. In 1818, an English manufacturer of the acid introduced the modern feature of using pyrites in the place of brimstone, while the Gay-Lussac tower in 1827 and the Glover tower in 1859 began to be applied as great improvements in the chamber process. Within about twenty years the contact process, employing platinized asbestos, has replaced the old chamber process to a large extent. It has the advantage of producing the concentrated acid, or the fuming acid, directly.

During our period the manufacture of sulphuric acid has increased enormously. Very large quantities of it have been used in connection with the Leblanc soda process in its rapid development. It came to be employed extensively for absorbing ammonia in the illuminating-gas industry, which was in its infancy one hundred years ago. New industries such as the manufacture of "superphosphates" as artificial fertilizers, the refining of petroleum, the manufacture of artificial dyestuffs and many other modern chemical products have greatly increased the demand for it, while its employment in the production of nitric and other acids, and for many other purposes not already mentioned, has been very great.

The manufacture of nitric acid has been greatly

extended during our period on account of its employment for producing explosives, artificial dyestuffs, and for many other purposes. Chile saltpeter became available for making it about 1852. This acid has been manufactured recently from atmospheric nitrogen and oxygen by combining them by the aid of powerful electric discharges. This process has been used chiefly in Norway where water-power is abundant, as it requires a large expenditure of energy. A still more recent method for the production of nitric acid depends upon the oxidation of ammonia by air with the aid of a contact substance, such as platinized asbestos.

The production of ammonia, which was very small a hundred years ago, has been vastly increased in connection with the development of the illuminating-gas industry and the employment of by-product coke ovens. This substance is very extensively used in refrigerating machines and also in a great many chemical operations, including the Solvay soda-process. Ammonium salts are of great importance also as fertilizers in agriculture. The conversion of atmospheric nitrogen into ammonia on a commercial scale is a recent achievement. It has been accomplished by heating calcium carbide, an electric-furnace product made from lime and coke, with nitrogen gas, thus producing calcium cyanamide, and then treating this cyanamide with water under proper conditions. Another method devised by Haber consists in directly combining nitrogen and hydrogen gases under high pressure with the aid of a contact substance.

Leblanc's method for obtaining sodium carbonate from sodium chloride by first converting the latter into the sulphate by means of sulphuric acid and then heating the sulphate with lime and coal in a furnace was invented as early as 1791, but it was not rapidly developed and did not gain a foothold in England until 1826 on account of a high duty on salt up to that time. Afterwards the process flourished greatly in connection with the sulphuric acid industry upon which it depended, and with the bleaching-powder industry which utilized the hydrochloric acid incidentally produced by it, and, of course, in connection with soap manufacture and many other industries in which the soda itself was employed.

About 1866 the Solvay process appeared as a rival to the Leblanc process. This depends upon the precipitation of sodium bicarbonate from salt solutions by means of carbon dioxide and ammonia, with the subsequent recovery of the ammonia. It has displaced the older process to a large extent, and it is carried on extensively in this country, for instance, at Syracuse, New York.

Other processes for soda depend upon the electrolysis of sodium chloride solutions. In this case caustic soda and chlorine are the direct products, and the chlorine thus produced and liquified by pressure in steel cylinders, has become an important commercial article.

In earlier times wood-ashes were the source of potash and potassium salts. Wurtz in the Journal (**10**, 326, 1850) suggested the availability of New Jersey greensand as a source of potash and showed how this mineral could be decomposed, but it does not appear that this mineral has ever been utilized for the purpose. About 1861 the German potash-salt deposits began to be developed, and these have since become the chief source of this material. At present many efforts are being made to obtain potassium compounds from other sources, such as brines, cement-kiln dust, and feldspar and other minerals but thus far the results have not satisfied the demand.

Conclusion.

This account of chemical progress has given only a limited view of small portions of the subject, because the amount of available material is so vast in comparison with the space allowed for its presentation. Since the Journal has published comparatively little organic chemistry, it was decided to make room for a better presentation of other things by giving only a brief discussion of this exceedingly active and important branch of the science. For similar reasons industrial and metallurgical chemistry, and other branches besides, in spite of their great growth and importance, have been neglected, except for some incidental references to them, and some account of a few of the more important industrial chemicals.

It appears that we have much reason to be proud of the

advances in chemistry that have been made during the Journal's period, and of the part that the Journal has taken in connection with them, and there seems to be no doubt that this progress has not diminished during more recent times.

The present tendency of chemical research is evidently towards a still greater development of organic chemistry, and an increased application of physics and mathematics to chemical theory and practice.

The very great improvements that have been made in chemical education, both in the number of students and the quality of instruction, during the period under discussion, and particularly in rather recent times, gives promise for excellent future progress.

Note.

[1] It appears that the most accurate experimental demonstration ever made of this law was that of E. W. Morley, published in the Journal (41, 220, 276, 1891). He showed that 2·0002 volumes of hydrogen combine with one volume of oxygen.

XI

A CENTURY'S PROGRESS IN PHYSICS

By LEIGH PAGE

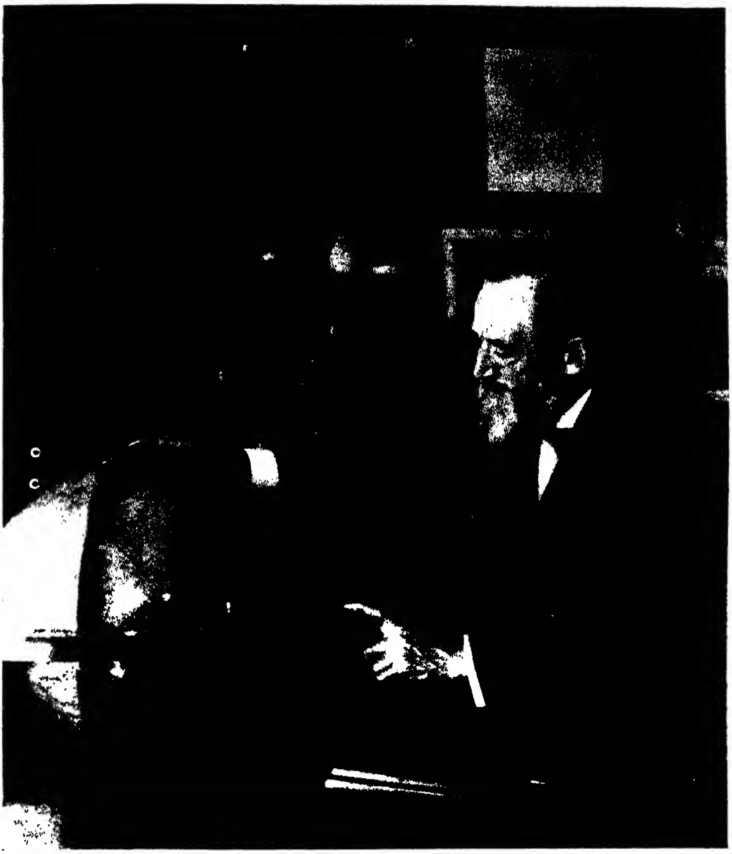
DYNAMICS.—At the beginning of the nineteenth century mechanics was the only major branch of physical science which had attained any considerable degree of development. Two centuries earlier, Galileo's experiments on the rate of fall of iron balls dropped from the top of the Leaning Tower of Pisa, had marked the origin of dynamics. He had easily disproved the prevalent idea that even under conditions where air resistance is negligible heavy bodies would fall more rapidly than light ones, and further experiments had led him to conclude that the increase in velocity is proportional to the *time* elapsed, and not to the *distance* traversed, as he had at first supposed. Less than a century later Newton had formulated the laws of motion in the same words in which they are given to-day. These laws of motion, coupled with his discovery of the law of universal gravitation, had enabled him to correlate at once the planetary notions which had proved so puzzling to his predecessors. His success gave a tremendous stimulus to the development and extension of the fundamental dynamical principles that he had brought to light, which culminated in the work of the great French mathematicians, Lagrange and Laplace, a little over a hundred years ago.

Newton's laws of motion, it must be remembered, apply only to a particle, or to those bodies which can be treated as particles in the problem under consideration. In his "Mécanique Analytique" Lagrange extended these principles so as to make it possible to treat the motion of a connected system by a method almost as simple as that contained in the second law of motion.

Instead of three scalar equations for each of the innumerably large number of particles involved, he showed how to reduce the ordinary dynamical equations to a number equal to that of the degrees of freedom of the system. This is made possible by a combination of d'Alembert's principle, which eliminates the forces due to the connections between the particles, and the principle of virtual work, which confines the number of equations to the number of possible independent displacements. The aim of Lagrange was to make dynamics into a branch of analysis, and his success may be inferred from the fact that not a single diagram or geometrical figure is to be found in his great work.

Celestial Mechanics.—Almost simultaneously with the publication of the "Mécanique Analytique" appeared Laplace's "Mécanique Céleste." Laplace's avowed aim was to offer a complete solution of the great dynamical problem involved in the solar system, taking into account, in addition to the effect of the sun's gravitational field, those perturbations in the motion of each planet caused by the approach and recession of its neighbors. So successful was his analysis of planetary motions that his contemporaries believed that they were not far from a complete explanation of the world on mechanical principles. Laplace himself was undoubtedly convinced that nothing was needed beyond a knowledge of the masses, positions, and initial velocities of every material particle in the universe in order to completely predetermine all subsequent motion.

The greatest triumph of these dynamical methods was to come half a century later. The planet Uranus, discovered in 1781 by the elder Herschel, was at that time the farthest known planet from the sun. But the orbit of Uranus was subject to some puzzling variations. After sifting all the known causes of these disturbances, Leverrier in France and Adams in England independently reached the conclusion that another planet still more remote from the sun must be responsible, and computed its orbit. Leverrier communicated to Galle of Berlin the results of his calculations, and during the next few days the German astronomer discovered Neptune within one degree of its predicted position!



H. A. Newton

We shall mention but one other achievement of the methods of celestial mechanics. Those visitors of the skies, the comets, which become so prominent only to fade away and vanish perhaps forever, had interested astronomers from the earliest times. Soon after the discovery of the law of gravitation, Newton had worked out a method by which the elements of a comet's orbit can be computed from observations of its position. It was found that the great majority of these bodies move in nearly parabolic paths and only a few in ellipses. Of the latter the most prominent is the brilliant comet first observed by Halley in 1681. It has reappeared regularly at intervals of seventy-six years; the last appearance in the spring of 1910 is no doubt well remembered by the reader. Kant had considered comets to be formed by condensing solar nebulæ, whereas Laplace had maintained that they originate in matter which is scattered throughout stellar space and has no connection with the solar system. A study of the distribution of inclinations of comet orbits by H. A. Newton (**16**, 165, 1878) of New Haven substantiated Laplace's hypothesis, and led to the conclusion that the periodic comets have been captured by the attraction of those planets near to which they have passed. Of these comets a number have comparatively short periods, and are found to have orbits which are in general only slightly inclined to those of the planets, and are traversed in the same direction. Moreover, the fact that the orbit of each of these comets comes very close to that of Jupiter made it seem probable that they have been attached to the solar system by the attraction of this planet. Further confirmation of this hypothesis was furnished by H. A. Newton's (**42**, 183 and 482, 1891) explanation of the small inclination of their orbits and the scarcity of retrograde motions among them.

In 1833 occurred one of the greatest meteoric showers of history. Olmstead (**26**, 132, 1834) and Twining (**26**, 320, 1834) of New Haven noticed that these shooting stars traverse parallel paths, and were the first to suggest that they must be moving in swarms in a permanent orbit. From an examination of all accessible records, H. A. Newton (**37**, 377, 1864; **38**, 53, 1864) was able to

show that meteoric showers are common in November, and of particular intensity at intervals of 33 or 34 years. He confidently predicted a great shower for Nov. 13th, 1866, which not only actually occurred but was followed by another a year later, showing that the meteoric swarm extended so far as to require two years to cross the earth's orbit. H. A. Newton (**36**, 1, 1888) in America and Adams in England took up the study of meteoric orbits with great interest, and the former concluded that these orbits are in every sense similar to those of the periodic comets, implying that a swarm of meteors originates in the disintegration of a comet. In fact Schiaparelli actually identified the orbit of the Perseids, or August meteors, with Tuttle's comet of 1862, and shortly after the orbit of the Leonids, or November meteors, was found to be the same as that of Tempel's comet.

Electromagnetism.—During the eighteenth century much interest had been manifested in the study of electrostatics and magnetism. Du Fay, Cavendish, Michell and Coulomb abroad and Franklin in America had subjected to experimental investigation many of the phenomena of one or both of these sciences, and in the early years of the nineteenth century Poisson developed to a remarkable extent the analytical consequences of the law of force which experiment had revealed. Both Laplace and he made much use of the function to which Green gave the name "potential" in 1828, and which is such a powerful aid in solving problems involving magnetism or electricity at rest.

Meantime electric currents had been brought under the hand of the experimenter by the discoveries of Galvani and Volta. Large numbers of cells were connected in series, and interest seemed to lie largely in producing brilliant sparks or fusing metals by means of a heavy current. Hare (**3**, 105, 1821) of the University of Pennsylvania constructed a battery consisting of two troughs of forty cells each, so arranged that the coppers and zincs can be lowered simultaneously into the acid and large currents obtained before polarization has a chance to interfere. This "deflagrator" was used to ignite

charcoal in the circuit, or melt fine wires, and was for some time the most powerful arrangement of its kind. That "galvanism" is something quite different from static electricity was the opinion of many investigators; Hare considered the heat developed to be the distinguishing mark of the electric current. He says: "It is admitted that the action of the galvanic fluid is upon or between atoms; while mechanical electricity when uncoerced, acts only upon masses. This difference has not been explained unless by my hypothesis, in which caloric, of which the influence is only exerted between atoms, is supposed to be a principal agent in galvanism."

Questioning minds were beginning to suspect that there must be some connection between electricity and magnetism. For lightning had been known to make magnets of steel knives and forks, and Franklin had magnetized a sewing needle by the discharge from a Leyden jar. Finally Oersted of Copenhagen undertook systematic investigation of the effect of electricity on the magnetic needle. His researches were without result until during the course of a series of lectures on "Electricity, Galvanism, and Magnetism" delivered during the winter of 1819-20 it occurred to him to investigate the action of an electric current on a magnetic needle. At first he placed the wire bearing the current at right angles to the needle, with, of course, no result; then it occurred to him to place it parallel. A deflection was observed, for to his surprise the needle insisted on turning until perpendicular to the wire.

Oersted's discovery that an electric current exerts a couple on a magnetic needle was followed a few months later by Ampère's demonstration before the French Academy that two currents flowing in the same direction attract each other, while two in opposite directions repel. The story goes that a critic attempted to belittle this discovery by remarking that as it was known that two currents act on one and the same magnet, it was obvious that they would act upon each other. Whereupon Arago arose to defend his friend. Drawing two keys out of his pocket he said, "Each of these keys attracts a magnet; do you believe that they therefore attract each other?"

A few years later Ampère showed how to express quantitatively the force between current elements, and indeed developed to a considerable degree the equivalence between a closed circuit carrying a current and a magnetic shell. So convincing was his analysis and so thorough his discussion of the subject, that Maxwell said of this memoir half a century later, "The whole, theory and experiment, seems as if it had leaped, full grown and full armed, from the brain of the 'Newton of electricity.' It is perfect in form and unassailable in accuracy; and it is summed up in a formula from which all the phenomena may be deduced, and which must always remain the cardinal formula of electrodynamics."

Shortly afterwards the dependence of a current on the conductivity of the wire used and the grouping of cells employed, was made clear by the work of Ohm. Many of his results were obtained independently by Joseph Henry (**19**, 400, 1831) of the Albany Academy, who described in 1831 a powerful electromagnet in which a great many coils of wire insulated with silk were wound around an iron core and connected in parallel with a single cell. He remarks in this paper that with long wires, as in the telegraph, many cells arranged in series should be used, whereas for several short wires connected in parallel a single cell with large plates is more efficient.

Current Induction.—Impressed by the fact that electric charges have the power of inducing other charges on neighboring conductors without coming into contact with them, Faraday was engaged in investigating the possibility of an analogous phenomenon in the case of electric currents. His idea at first seems to have been that a current should induce another current in any closed conducting circuit which happens to be in its vicinity. Experiment readily showed the falsity of this conception, but a brief deflection of the galvanometer in the secondary circuit was noticed at the instant of making and breaking the current in the primary. Further experiments showed that thrusting a permanent steel magnet into a coil connected to a galvanometer caused the needle to deflect. In fact Faraday's report to the Royal Society on November 24th, 1831, contains a com-

plete account of all experimental methods available for inducing a current in a closed circuit.

While Faraday is entitled to credit for the discovery of current induction by virtue of the priority of his publication, it must not pass unnoticed that Henry obtained many of the same experimental results independently and some even earlier. Henry was at this time instructor in mathematics at the Albany Academy, and seven hours of teaching a day made it well-nigh impossible to carry on original research except during the vacation month of August. As early as the summer of 1830 he had wound 30 feet of copper wire around the armature of a horseshoe electromagnet and connected it to a galvanometer. When the magnet was excited, a momentary deflection was observed. "I was, however, much surprised," he says, "to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction, when the battery was withdrawn from the acid, and again deflected to the west when it was re-immersed." In addition a deflection was obtained by detaching the armature from the magnet, or by bringing it again into contact. Had the results of these experiments been published promptly, America would have been entitled to credit for the most important discovery of the greatest of England's many great experimenters. But Henry desired first to repeat his experiments on a larger scale, and while new magnets were being constructed, the news of Faraday's discovery arrived. This occasioned hasty publication of the work already done in an appendix to volume **22**, 1832, of the Journal.

At almost the same time Henry made another important discovery and this time he was anticipated by no other investigator in making public his results. In the paper already referred to he describes the phenomenon known to-day as self-induction. "When a small battery is moderately excited by diluted acid and its poles, which must be terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken; but if a wire thirty or forty feet long be used,

instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury a vivid spark is produced. . . . The effect appears somewhat increased by coiling the wire into a helix; it seems to depend in some measure on the length and thickness of the wire; I can account for these phenomena only by supposing the long wire to become charged with electricity which by its reaction on itself projects a spark when the connection is broken."

Soon after, Henry went to Princeton and there continued his experiments in electromagnetism. No difficulty was experienced in inducing currents of the third, fourth and fifth orders by using the first secondary as primary for yet another secondary circuit, and so on (**38**, 209, 1840). The directions of these currents of higher orders when the primary is made or broken proved puzzling at first, but were satisfactorily explained a year later (**41**, 117, 1841). In addition induced currents were obtained from a Leyden jar discharge. Faraday failed to find any screening effect of a conducting cylinder placed around the primary and inside the secondary. Henry examined the matter, and found that the screening effect exists only when the induced current is due to a make or break of the primary circuit, and not when it is caused by motion of the primary.

Henry's work was mainly descriptive; it remained for Faraday to develop a theory to account for the phenomena discovered and to prepare the way for quantitative formulation of the laws of current induction. This he did in his representation of a magnetic field by means of lines of force; a conception which he found afterwards to be equally valuable when applied to electrostatic problems. Every magnet and every current gives rise to these closed curves; in the case of a magnet they thread it from south pole to north, while a straight wire bearing a current is surrounded by concentric rings. The connection between lines of force and the induction of currents is contained in the rule that a current is induced in a closed circuit only when a change takes place in the number of lines of force passing through it. Furthermore the dependence of the current strength on the

conductivity of the wire employed has led to recognition of the fact that it is the electromotive force and not the current itself which is conditioned by the change in magnetic flux.

Great interest was attached to the utilization of the newly discovered forces of electromagnetism. In 1831 Henry (**20**, 340, 1831) described a reciprocating engine depending on magnetic attraction and repulsion, and C. G. Page (**33**, 118, 1838; **49**, 131, 1845) devised many others. The latter's most important work, however, was the invention of the Ruhmkorff coil. In 1836 (**31**, 137, 1837) he found the strongest shocks to be obtained from a secondary coil of many windings forming a continuation of a primary of half the number of turns. His perfection of the self-acting circuit breaker (**35**, 252, 1839) widened the usefulness of the induction coil, and his substitution of a bundle of iron wires for a solid iron core (**34**, 163, 1838) greatly increased its efficiency.

Conservation of Energy.—Perhaps the most important advance of the nineteenth century has been the establishment of the principle of conservation of energy. Despite the fact that the "principe de la conservation des force vives" had been recognized by the French mathematicians of the early part of the century, the application of this principle even to purely mechanical problems was contested by some scientists. Through the early numbers of the Journal runs a lively controversy as to whether there is not a loss of power involved in imparting momentum to the reciprocating parts of a steam engine only to check the motion later on in the stroke. Finally Isaac Doolittle (**14**, 60, 1828), of the Bennington Iron Works, ends the discussion by the pertinent remark: "If there be, as is contended by one of your correspondents, a loss of more than one third of the power, in transforming an alternating rectilinear movement into a continuous circular one by means of a crank, I should like to be informed what would be the effect if the proposition were reversed, as in the case of the common saw mill, and in many other instances in practical mechanics."

A realization of the equivalence of heat and mechanical work did not come until the middle of the century, in

spite of the conclusive experiments of the American Count Rumford and the English Davy before the year 1800. So firmly enthroned was the caloric theory, according to which heat is an indestructible fluid, that evidence against it was given scant consideration. In fact the success of the analytical method introduced by Fourier in 1822 for the solution of problems in conduction of heat only added to the difficulties of the adherents of the kinetic theory. But recognition of heat as a form of energy was on the way, and when it came it made its appearance almost simultaneously in half a dozen different places. Perhaps Robert Mayer of Heilbronn was the first to state explicitly the new principle. His paper "On the Forces of Inorganic Nature" was refused publication in Poggendorff's Annalen, but fared better at the hands of another editor. During the next few years Joule determined the mechanical equivalent of heat experimentally by a number of different methods, some of which had already been devised by Carnot. Of those he used, the most familiar consists in churning up a measured mass of water by means of paddles actuated by falling weights and calculating the heat developed from the rise in temperature. However, the work of the young Manchester brewer received little attention from the members of the British Association before whom it was reported until Kelvin showed them its significance and attracted their interest to it. Meanwhile Helmholtz had completed a very thorough disquisition on the conservation of energy not only in dynamics and heat but in other departments of physics as well. His paper on "Die Erhaltung der Kraft" was frowned upon by the members of the Physical Society of Berlin before whom he read it, and received the same treatment as Mayer's from the editor of Poggendorff's Annalen. Helmholtz's "Kraft," like the "vis viva" of other writers, is the quantity which Young had already christened energy. Not many years elapsed, however, until the convictions of Mayer, Joule, Kelvin and Helmholtz became the most clearly recognized of all physical principles. As early as 1850 Jeremiah Day (**10**, 174, 1850), late president of Yale College, admitted the improbability of constructing

a machine capable of perpetual motion, even though the "imponderable agents" of electricity, galvanism and magnetism be utilized.

Thermodynamics.—The importance of the principle of conservation of energy lies in the fact that it unites under one rule such diverse phenomena as gravitation, electro-magnetism, heat and chemical action. Another principle as universal in its scope, although depending upon the coarseness of human observations for its validity rather than upon the immutable laws of nature, was foreshadowed even before the first law of thermodynamics, or principle of conservation of energy, was clearly recognized. This second law was the consequence of efforts to improve the efficiency of heat engines. In 1824 Carnot introduced the conception of cyclic operations into the theory of such engines. Assuming the impossibility of perpetual motion, he showed that no engine can have an efficiency greater than that of a reversible engine. Finally Clausius expressed concisely the principle toward which Carnot's work had been leading, when he asserted that "it is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature." Kelvin's formulation of the same law states that "it is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects."

The consequences of the second law were rapidly developed by Kelvin, Clausius, Rankine, Barnard (**16**, 218, 1853, *et seq.*) and others. Kelvin introduced the thermodynamic scale of temperature, which he showed to be independent of such properties of matter as condition the size of the degree indicated by the mercury thermometer. This scale, which is equivalent to that of the ideal gas thermometer, was used subsequently by Rowland in his exhaustive determination of the mechanical equivalent of heat by an improved form of Joule's method. He found different values for different ranges in temperature, showing that the specific heat of water is by no means constant. Since then electrical methods

of measuring this important quantity have been used to confirm the results of purely mechanical determinations.

The definition of a new quantity, entropy, was found necessary for a mathematical formulation of the second law of thermodynamics. This quantity, which acts as a measure of the unavailability of heat energy, was given a new significance when Boltzmann showed its connection with the probability of the thermodynamic state of the substance under consideration. If two bodies have widely different temperatures, a large amount of the heat energy of the system is available for conversion into mechanical work. From the macroscopic point of view this is expressed by saying that the entropy is small, or if the motions of the individual molecules are taken into account, the probability of the state is low. The interpretation of entropy as the logarithm of the thermodynamic probability has thrown much light on the meaning of this rather abstruse quantity. Gibbs's "Elementary Principles in Statistical Mechanics" treats in detail the fundamental assumptions involved in this point of view, its limitations and its consequences. In his "Equilibrium of Heterogeneous Substances"[1] he had already extended the principle of thermal equilibrium to include substances which are no longer homogeneous. The value of the chemical potential he introduced determines whether one phase is to gain at the expense of another or lose to it. It is unfortunate that the analytical rigor and austerity of his reasoning combined with lack of mathematical training on the part of the average chemist, delayed true appreciation of his work and full utilization of the new field which he opened up.

Liquefaction of Gases.—Meanwhile the problem of liquefying gases was attracting much attention on the part of experimental physicists. Faraday had succeeded in making liquid a number of substances which had hitherto been known only in the gaseous state. His method consists in evolving the gas from chemicals placed in one end of a bent tube, the other end of which is immersed in a freezing mixture. The high pressure caused by the production of the gas combined with the low temperature is sufficient to bring about liquefaction

in many cases. Failure with other more permanent gases was unexplained until the researches of Andrews in 1863 showed that no amount of pressure will produce liquefaction unless the temperature is below a certain critical value. The method of reducing the temperature in use to-day depends on a fact discovered by Kelvin and Joule in connection with the free expansion of a gas. These investigators allowed the gas to escape through a porous plug from a chamber in which the pressure was relatively high. With the single exception of hydrogen, the effect of the sudden expansion is to cool the gas, and even with it cooling is found to take place after the temperature has been made sufficiently low. By this method all known gases have been liquefied. Helium, with a boiling point of —269°C., or only 4°C. above the absolute zero, was the last to be made a liquid, finally yielding to the efforts of Kammerlingh Onnes in 1907. This investigator[2] finds that at temperatures near the absolute zero the electrical conductivity of certain substances undergoes a profound modification. For example, a coil of lead shows a superconductivity so great that a current once started in it persists for days after the electromotive force has ceased to act.

Electrodynamics.—Faraday's representation of electric and magnetic fields by lines of force had been of great value in predicting the results of experiments in electromagnetism. But a more mathematical formulation of the laws governing these phenomena was needed in order to make possible quantitative development of the theory. This was supplied by Maxwell in his epoch-making treatise on "Electricity and Magnetism." Starting with electrostatics and magnetism, he gives a complete account of the mathematical methods which had been devised for the solution of problems in these branches of the subject, and then turning to Ampère's work he shows how the Lagrangian equations of motion lead to Faraday's law if the single assumption is made that the magnetic energy of the field is kinetic. In the treatment of open circuits Maxwell's intuition led to a great advance, the introduction of the displacement current. Consider a charged condenser, the plates of which are suddenly con-

nected by a wire. A current will flow through the wire from the positively charged plate to the negative, but in the gap between the two plates the conduction current is missing. So convinced was Maxwell that currents must always flow in *closed* circuits, that he postulated an electrical displacement in the medium between the plates of a charged condenser, which disappears when the condenser is short-circuited. Thus even in the so-called open circuit the current flows along a closed path.

Maxwell's theory of the electromagnetic field is based essentially on Faraday's representation by lines of force of the strains and stresses of a universal medium. So it is not surprising that he was led to a consideration of the propagation of waves through this medium. The introduction of the displacement current made the form of the electrodynamic equations such as to yield a typical wave equation for space free from electrical charges and currents. Moreover, the disturbance was found to be transverse, and its velocity turned out to be *identical with that of light.* The conclusion was irresistible. That light could consist of anything but electromagnetic waves of extremely short length was inconceivable. In fact so certain was Maxwell of this deduction from theory that he felt it altogether unnecessary to resort to the test of experiment. For the electromagnetic theory explained so many of the details which had been revealed by experiments in light, that no doubt of its validity could be entertained. Even dispersion received ready elucidation on the assumption that the dispersing medium is made up of vibrators having a natural period comparable with that of the light passing through it.

Maxwell's book was published in 1873. Fifteen years later, Hertz,[3] at the instigation of Helmholtz, succeeded in detecting experimentally the electromagnetic waves predicted by Maxwell's theory. His oscillator consisted of two sheets of metal in the same plane, to each of which was attached a short wire terminating in a knob. The knobs were placed within a short distance of each other, and connected to the terminals of an induction coil. By reflection standing waves were formed, and the positions of nodes and loops determined by a detector composed of a movable loop of wire containing an air gap. Thus the

James Clerk Maxwell.

wave length was measured. Hertz calculated the frequency of his radiator from its dimensions, and then computed the velocity of the disturbance. In spite of an error in his calculations, later pointed out by Poincaré, he obtained very nearly the velocity of light for waves traveling through air, but a velocity considerably smaller for those propagated along wires. Subsequent work by Lecher, Sarasin and de la Rive, and Trowbridge and Duane (**49**, 297, 1895; **50**, 104, 1895) cleared up this discrepancy, and showed the velocity to be in both cases identical with that of light. The last-named investigators increased the size of the oscillator until it was possible to measure the frequency by photographing the spark in the secondary with a rotating mirror. The positions of nodes and loops were obtained by means of a bolometer after the secondary had been tuned to resonance with the vibrator. The velocity thus found for electromagnetic waves along wires is within one-tenth of one percent of the accepted value of the velocity of light. Hertz's later experiments showed that waves in air suffer refraction and diffraction, and he succeeded in polarizing the radiation by passing it through a grating constructed of parallel metallic wires.

In order to satisfy the law of action and reaction, it is found necessary to attribute a quasi-momentum to electromagnetic waves. When a train of such waves is absorbed, their momentum is transferred to the absorbing body, while if they are reflected an impulse twice as great is imparted. This consequence of theory, foreseen by Maxwell and developed in detail by Poynting, Abraham and Larmor, has been verified by the experiments of Lebedew, and Nichols and Hull.[4] The latter used a delicate torsion balance from which was suspended a couple of silvered glass vanes. In order to eliminate the effect of impulses imparted by the molecules of the residual gas, such as Crookes had observed in his radiometer, readings were made at many different pressures and the ballistic rather than the static deflection recorded. After the pressure produced by light from a carbon arc had been measured, the intensity of the radiation was determined with a bolometer. Preliminary experiments indicated the existence of a pressure of the order

expected, and later more careful measurements showed good quantitative agreement with theory. This pressure had already found an important application in Lebedew's explanation of the solar repulsion of comet's tails. These tails are made up of enormous swarms of very minute particles, and as the comet swings around the sun they suffer a repulsion due to the pressure of the intense solar radiation which counteracts the sun's gravitational attraction. Hence the tail, instead of following after the comet in its orbit, points in a direction away from the sun.

Some uncertainty existed as to whether a convection current produces a magnetic field. A compass needle is deflected by a current from a Daniell cell; is the same effect obtained when a conductor is charged electrostatically and then whirled around the needle by means of an insulating handle? The experimental difficulties involved in settling this question are realized when the enormous difference between the electrostatic and electromagnetic units of current is taken into consideration. For a sphere one centimeter in radius, charged to a potential of 20,000 volts, and revolving in a circle sixty times a second, constitutes a current of little over a millionth of an ampere.

This problem was undertaken by Rowland (**15**, 30, 1878) in Helmholtz's laboratory at Berlin in 1876. A hard rubber disk coated on both sides with gold was charged and rotated about a vertical axis at a rate of sixty revolutions a second. On reversing the sign of the electrification on the disk, the astatic needle hung above its center showed a deflection of over five millimeters. The current was calculated in electrostatic units from the charge on the disk and its rate of motion, and in electromagnetic units from the magnetic deflection. The ratio of these two quantities gave fair agreement with its theoretical value, the velocity of light.

Although the result of this experiment was confirmed by Rowland and Hutchinson in 1889, Crémieu was convinced by an investigation carried out at Paris in 1900 that the Rowland effect did not exist. Consequently further repetition of the experiment was desirable. So the following year Adams (**12**, 155, 1901) arranged two

rings of eight spheres each so that they could be rotated about their common axis from fifty to sixty times a second. One set of spheres was connected by brushes to the positive pole of a battery of 20,000 volts, the other to the negative pole. The deflection of a nearby magnetometer needle was observed when the electrification of the two rings was reversed, and from the reading so obtained the ratio of the electromagnetic to the electrostatic unit of current computed. This quantity was found to differ from the velocity of light by only a few percent. This experiment and the even more exhaustive investigations carried out by Pender, both independently and in collaboration with Crémieu, finally convinced the scientific world that a convection current produces the same magnetic field as a conduction current of the same magnitude.

In discussing the ponderomotive force experienced in a magnetic field by a conductor through which a current is passing, Maxwell had said, "It must be carefully remembered, that the mechanical force which urges a conductor carrying a current across the lines of magnetic force, acts, not on the electric current, but on the conductor which carries it." Hall (19, 200, 1880), one of Rowland's students, questioned this statement, and determined to put it to the test of experiment. Efforts to find an increase in the resistance of a wire placed at right angles to the lines of magnetic force were unsuccessful. So the current was passed through a moderately broad strip of gold leaf and the effect of the magnetic field on the equipotential lines investigated. The results obtained confirmed Hall's belief that the force exerted by the field acts on the current itself, and is transmitted through it to the conductor. Further investigation (**20**, 161, 1880) revealed the same deflection of equipotential lines in thin strips of other metals, although the effect was found to be reversed in iron.

During the closing years of the nineteenth century occurred three events of far reaching importance. The electron was isolated, and its charge and mass measured by J. J. Thomson in England; X-rays were discovered by Röntgen in Germany; and the first indications of radioactivity were found by Becquerel in France. The first two are certainly to be attributed largely to the

great advances which had been made in obtaining high vacua, and the last two might not have occurred so soon had it not been for the photographic plate.

The Electron.—The atomic theory of electricity dates from the time of Faraday. His experiments on electrolysis showed that each monovalent atom or radical, whatever its nature, carries the same charge, each bivalent ion a charge twice as great. Only a lack of knowledge of the number of atoms in a gram of the dissociated salt prevented him from calculating the value of the elementary charge. As the discharge of electricity through gases at low pressures became a subject for experimental investigation, another line of approach to the study of the atom of electricity was opened up. As early as the seventies Hittorf and Goldstein had observed that a shadow is cast by a screen placed in front of the cathode of a Crookes tube. Varley suggested that the cathode rays producing the shadow consist of "attenuated particles of matter, projected from the negative pole by electricity." The discovery that these rays are deflected by a magnetic field led English physicists to the conclusion that they must be composed of charged particles, and the direction of the deflection was such as to require the charge to be negative. Hertz contested this view on the ground that his experiments showed the rays to be unaffected by an electrostatic field, and suggested that they consist of etherial disturbances. Finally Perrin succeeded in passing the rays into a metal cylinder which received from them a negative charge, and Lenard showed how excessively minute these negatively charged particles must be by actually passing them through a thin sheet of aluminium in the wall of a vacuum tube, and detecting their presence in the air outside. Conclusive information as to the nature of the electron, as it was named by Johnstone Stoney, was supplied by the classic experiments of J. J. Thomson.[5] First he showed that Hertz's failure to find a deflection when a stream of electrons passes between the plates of a charged condenser was due to the screening effect of the gaseous ions produced by the discharge. With a much more highly evacuated tube he found no difficulty in obtaining a deflection in an electrostatic field. By using crossed electric and magnetic

fields the deflection produced by one was just balanced by that caused by the other, and from the field strengths employed both the velocity of the particles and the ratio $\frac{e}{m}$ of charge to mass was calculated. The former was found to be about one-tenth the velocity of light, but the most startling result of the experiment was that the same value of $\frac{e}{m}$ was obtained no matter what residual gas was contained in the tube or of what metal the cathode was made.

To calculate e and then m other methods are necessary. C. T. R. Wilson has shown that in supersaturated air, water drops form easily on charged molecules, and that negative ions are more effective in causing condensation than positive ones. By making use of the results of this research Thomson has been able to measure the elementary charge. For suppose a stream of negative ions to pass through supersaturated air. A little drop forms on each charged particle, and the cloud of condensed vapor settles to the bottom of the vessel. The charge carried and the mass of water deposited can be measured directly. Stokes' law for the rate of fall of a minute particle through a gaseous medium enables the average size of the drops to be computed from the observed rate of descent of the cloud. Hence the number of drops formed and the charge carried by each follows at once. H. A. Wilson improved the method by noting the effect of an electric field upon the rate of fall of the charged drops, and subsequent experiments undertaken by Millikan[6] have been of such a character as to enable him to follow the motion of a single drop. Instead of water, the latter uses oil drops less than one ten-thousandth of a centimeter in diameter. A drop, after one or more electrons have attached themselves to it, is actually *weighed* in terms of the charge on its surface by applying an upward electric force just sufficient to balance the force of gravity. Then its weight is independently obtained from the density of the oil and the radius of the drop as determined by the rate of fall when the electric field is absent. Comparison of these two expressions gives $4{\cdot}774(10)^{-10}$ electrostatic units for the

elementary charge. Combining this result with the value of $\frac{e}{m}$ found by Thomson, the mass of the electron comes out to be about one eighteen-hundredth that of an atom of the lightest known element, hydrogen.

That the electron is a fundamental constituent of all matter is attested by the fact that charge and mass are the same regardless of the source or manner of production. Whether emitted by a heated metal, under the action of ultra-violet light, from a radioactive substance, by a body exposed to X-rays, as a result of friction, it is the same negatively charged particle that constitutes the cathode ray of the discharge tube. Moreover, it makes its effect felt indirectly in many other phenomena, and from an investigation of some of these the ratio of charge to mass can be determined independently. Of such perhaps the most interesting is the Zeeman effect.

Spectroscopy.—Early in the nineteenth century Fraunhofer had observed that the solar spectrum is crossed by a large number of dark lines. Their presence was unexplained until in 1859 Kirchhoff and Bunsen showed "that a colored flame, the spectrum of which contains bright sharp lines, so weakens rays of the color of these lines when they pass through it, that dark lines appear in place of bright lines as soon as there is placed behind the flame a light of sufficient intensity, in which the lines are otherwise absent." For intra-atomic oscillators must have the natural frequency of the radiation which they emit, and consequently resonance will take place when they are exposed to rays of this frequency coming from an outside source, and selective absorption ensue. By comparing the bright lines in the spectra of metallic vapors made luminous by a gas flame with the dark lines in the sun's spectrum these investigators showed that many of the common terrestrial elements exist in the sun. The interest in spectroscopy grew rapidly. The excellent diffraction gratings made by Rutherfurd were succeeded by the superior concave gratings of Rowland. In 1877 Draper (**14**, 89; 1877) announced the discovery of the bright lines of oxygen in the solar spectrum, but his interpretation of his photographs has not been corroborated by the work of later investigators. Langley (**11**,

401, 1901), by the aid of his newly invented bolometer, succeeded in detecting the emission of energy from the sun in the infra-red in amounts far exceeding that contained in the visible spectrum. In 1842 Doppler drew attention to the fact that motion of the source should cause a displacement of the spectral lines, the shift being to the blue if the light is approaching and to the red if it is receding, and a few years later Fizeau suggested the application of Doppler's principle to the measurement of the velocity of a star moving in the line of sight. Thus the spectroscope has been able to supply one of the deficiencies of the telescope, and the two together are sufficient to reveal all components of stellar motion. When spectra formed by light from the sun's limb and from its center are compared, the same effect reveals the rotation of the sun about its axis. (C. S. Hastings, **5**, 369, 1873; C. A. Young, **12**, 321, 1876.)

Further Evidence of the Electron.—In 1845 Faraday discovered a rotation of the plane of polarization when light passes in the direction of the lines of force through a piece of glass placed between the poles of an electromagnet. Examination of the spectrum from a glowing vapor situated between the poles of a magnet, however, failed to reveal any effect of the field. The latter problem was attacked anew by Zeeman[7] in 1896, and with the aid of the improved appliances of modern science he succeeded in detecting a broadening of the lines. Later experiments with more powerful apparatus resolved these broadening lines into several components.

Lorentz[8] showed at once how the electron theory furnishes an explanation of the Zeeman effect. He found that when the source is viewed at right angles to the lines of magnetic force, a spectral line should be split into three components. Of these he predicted that the middle, or undisplaced component, would be found to be polarized at right angles to the direction of the field, and the other components parallel to the field. When the light proceeds from the source in a direction parallel to the magnetic lines of force, two components only should be formed, and these should be circularly polarized in opposite senses. Moreover, from the separation of the components can be calculated the ratio of charge to mass

of the electronic vibrator which is responsible for the emission of radiant energy. Zeeman's experiments confirmed Lorentz's theory in every detail, and yielded a value of $\frac{e}{m}$ in substantial agreement with that obtained for cathode rays. Subsequent research, however, has shown that in many cases more components are found than the elementary theory calls for. Hale has detected the Zeeman effect in light from sun spots, proving that these blemishes on the sun's face are vortices caused by whirling swarms of electrified particles. Recently Stark and Lo Surdo have found a similar splitting up of lines in the spectrum formed by light from canal rays (rays of positively charged particles) passing through an intense electric field. This phenomenon has as yet received no adequate explanation.

On discovering that an electric current is capable of producing a magnetic field, Ampère had suggested that the magnetic properties of such substances as iron might be explained on the assumption of molecular currents. The electron theory considers these currents to be due to the revolution, inside the atom, of negatively charged particles about an attracting nucleus. It occurred to Richardson that this motion should give the atom the properties of a gyrostat. Hence if an iron bar be rotated about its axis, the atoms should orient themselves so as to make their axes more nearly parallel to the axis of rotation. Thus its rotation should cause the bar to become a magnet. Barnett[9] has tested this hypothesis, and has found the effect Richardson had predicted. From the strength of the magnetization produced, the value of $\frac{e}{m}$ can be computed. Barnett finds a value somewhat smaller than that for cathode rays, but of the right order of magnitude and sign. Einstein and De Haas have detected the inverse of this effect, i. e., the rotation of an iron rod when it is suddenly magnetized.

X-Rays.—In 1895, on developing a plate which had been lying near a vacuum tube, Röntgen[10] was surprised to find distinct markings on it. As the plate had never been exposed to light, it was necessary to suppose the

effect to be due to some new and unknown type of radiation. Further investigation showed that this radiation originates at the points where cathode rays impinge on the glass walls of the tube. Besides being able to pass with ease through all but the most dense material objects X-rays were found to have the power of ionizing gases through which they pass and ejecting electrons from metal surfaces against which they strike. The points at which these electrons are produced are in turn the sources of secondary X-rays whose properties are characteristic of the metal from which they come.

Röntgen's discovery excited intense interest among laymen as well as in scientific circles. Of the many X-ray photographs taken, those of Wright (**1**, 235, 1896) of Yale were the first to be produced in this country. His experiments were made immediately on receipt of the news of Röntgen's research, and resulted in the publication of a number of photographs showing the translucency for these rays of paper, wood, and even aluminium.

As X-rays are undeviated by electric or magnetic fields, Schuster, and later Wiechert and Stokes, suggested that they might be electromagnetic waves of the same nature as light, but much shorter and less regular. The great objection to this hypothesis was the failure either to refract or diffract these rays. In fact Bragg contended that they were not etherial disturbances at all, but consisted of neutral particles moving with very high velocities. Finally Laue[11] demonstrated their undulatory nature by showing that diffraction took place under proper conditions. Just as the distance between adjacent lines of a grating must be comparable to the wave length of light for a spectrum to be formed, a periodic structure with a grating space of their very much shorter wave length is necessary to diffract X-rays. Such a structure is altogether too fine to be made by human tools. Nature, however, has already prepared it for man's use. The distance between the atoms of a crystal is just right to make it an excellent X-ray grating, and Laue had no difficulty in obtaining diffraction patterns when Röntgen rays were passed through a block of zincblende. The distance between adjacent atoms of this

cubic crystal can be computed at once from its density and molecular weight, and then the wave length of the radiation calculated from the deviation suffered. In this way X-rays are found to have a length less than one thousandth as great as visible light. Further study of this phenomenon, particularly by the two Braggs, father and son, has revealed many of the structural details of more complicated crystals.

The most significant investigation in the field opened up by Laue's discovery is that undertaken by Moseley[12] only a couple of years before he lost his life in the trenches at Gallipoli. Using many different metals as anticathodes in a vacuum tube, he measured the frequencies of the characteristic rays emitted. He found that if the elements are arranged in order of increasing atomic weight, the square roots of the characteristic frequencies form an arithmetical progression. If to each element is assigned an integer, beginning with one for hydrogen, two for helium, and so on, the square root of the frequency of the characteristic radiation is found to be proportional to this atomic number. Even though Uhler has shown recently that over wide ranges Moseley's law does not hold within the limits of experimental error, there is undoubtedly much significance to be attached to this simple relation.

Radioactivity.—The year following the discovery of X-rays, Becquerel found that a photographic plate is similarly affected by radiations from uranium salts. Two years later the Curies separated from pitchblende the very active elements polonium and radium. Passage of the rays from these substances through electric and magnetic fields revealed the existence of three types. The alpha rays have been shown by Rutherford and his co-workers to be positively charged helium atoms; the beta rays are very rapidly moving electrons; and the gamma rays are electromagnetic pulses of the same nature as X-rays but somewhat shorter. In 1902 Rutherford and Soddy advanced the theory of atomic disintegration, according to which the emission of a ray is an indication of the breaking down of the atom to a simpler form. Thus in the radioactive substances there is going on before our

eyes a continual transformation of one element into another, a change, by the way, which appears to be in no slightest degree either hastened or delayed by changes in temperature (H. L. Bronson, **20**, 60, 1905) or external electrical condition of the radioactive element. Uranium is the progenitor of a long line of descendants, of which radium was supposed for some time to be the first member. Boltwood (**25**, 365, 1908) of Yale, however, showed that the slow growth of radium in uranium solutions is incompatible with this assumption, and soon isolated an intermediate product which he named ionium. Radium itself disintegrates into a gas known as radium emanation, which in turn gives rise to a succession of other products. Analyses by Boltwood (**23**, 77. 1907) of radioactive minerals from the same locality show such a constant ratio between the amounts of uranium and lead present that it is natural to conclude that lead is the end product of the series. This hypothesis is confirmed by the fact that the oldest rocks show relatively the greatest amounts of this element.

In addition to the Ionium-Radium series two others have been discovered. Of these Boltwood's (**25**, 269, 1908) investigations seem to indicate that the one which starts with actinium is a collateral branch of the radium series and comes from the same parent uranium. The other begins with thorium and comprises ten members. As yet the end products of the actinium and thorium series have not been identified, although there is some reason for believing that an isotope of lead may be the final member of the latter.

As the amount of a radioactive element which disintegrates in a given time is proportional to the total mass present, an infinite time would be required for the substance to be completely transformed. Hence the life of such an element is measured by the half value period, or time taken for half the initial mass to disintegrate. This time varies widely for different radioactive substances, ranging from a small fraction of a second for actinium A to five billion years for uranium. Boltwood's (**25**, 493, 1908) original determination of the life of radium from the rate of its growth in a solution containing ionium gave 2000 years as its result, although

recent measurements by Miss Gleditsch (**41**, 112, 1916) agree more closely with the value 1760 years obtained by Rutherford and Geiger from the number of alpha particles emitted.

Under the action of X-rays or the radiations from radioactive substances, gases acquire a conductivity which has been attributed by Thomson and Rutherford to the formation of ions. Zeleny has found that ions of opposite sign have somewhat different mobilities in an electric field, and experiments of Wellisch (**39**, 583, 1915) show that at low pressures some of the negative ions are electrons. T. S. Taylor (**26**, 169, 1908 *et seq.*) and Duane (**26**, 464, 1908) have investigated the ionization produced by alpha particles, and Bumstead (**32**, 403, 1911 *et seq.*) has studied the emission of electrons from metals which are bombarded by these rays. The investigations of Franck and Hertz, and McLennan and Henderson, show a significant relation between the ionizing potential (energy which must be possessed by an electron in order to produce an ion on colliding with an atom) and a quantity, to be considered later in more detail. which has been introduced by Planck into the theory of radiation.

Methods of Science.—Scientific progress seems to follow a more or less clearly defined path. Experimentation brings to light the hidden processes of nature, and hypotheses are advanced to correlate the facts discovered. As more and more phenomena are found to fit into the same scheme, the hypotheses at first proposed tentatively, although often only after extensive alterations, become firmly established as theories. Finally there may appear a fundamental clash between two theories, each of which in its respective domain seems to represent the only possible manner in which a large group of phenomena can be correlated. The maze becomes more perplexing at every step. At last a genius appears on the scene, approaches the problem from a new and unsuspected point of view, and the paradox vanishes. Such changes in point of view are the milestones which mark the progress of science. That science is stagnant whose only function is to collect, classify and correlate vast stores of experimental data. The sign of vitality is the existence of clearly defined and fundamental problems

any possible solution of which seems irreconcilable with the most basic truths of the science in question. The greater the paradox grows, the more certain the advent of a new point of view which will bring one step nearer the comprehensive picture of nature which is the goal of natural philosophy

The Ether.—From the earliest times philosophers have been attracted by the possibility of explaining physical phenomena in terms of an all-pervading medium. So strong had this tendency become by the middle of the nineteenth century that the English school of physicists were attributing rigidity, density and nearly all the properties of material media to the ether. In fact most physicists seemed to have forgotten that no experiment had ever given *direct* evidence of the existence of such a medium. Not until the first decade of the twentieth century was it realized that the experimental evidence actually pointed in quite the opposite direction, and that a new point of view was needed in dealing with those phenomena of light and electromagnetism which had been previously described in terms of a universal medium. Some account of the development of the ether theory and of the origin and growth of the point of view which has its principal exemplification in the principle of relativity is essential for an understanding of present tendencies in formulating a philosophic basis for scientific thought.

In the time of Newton and for a century after there was much controversy between the adherents of two irreconcilable theories of light. Hooke had suggested that light is a wave motion traveling through a homogeneous medium which fills all space, and Huygens had shown that the law of refraction can be deduced at once from this hypothesis if it is assumed that the velocity of light in a transparent body is less than that in free ether. However, Newton, impressed by the fact that a ray obtained by double refraction in Iceland spar differs from a ray of ordinary light just as a rod of rectangular cross section differs from one of circular cross section, and seeing no way of explaining this dissymmetry in terms of a wave motion analogous to longitudinal sound waves, adhered to the view that light consists of infinitesimal

particles shot out from the luminous body with enormous velocities. So great was his reputation on account of his discoveries in other fields that this theory of light held sway among his contemporaries and successors until the labors of Young and Fresnel at the beginning of the nineteenth century definitely established the undulatory theory. However, in spite of the fact that a corpuscular theory of light made the assumption of an ether unnecessary in so far as the simpler of the observed phenomena are concerned, even Newton postulated the existence of such a medium, partly in order to explain the more complicated results of experiments in light, and partly in order to provide a vehicle for the propagation of gravitational forces.

Now an ether, if it is to explain anything at all, must have at least some of the simpler properties of material media. The most fundamental of these, perhaps, is position in space. As a first approximation in explaining optical phenomena on the earth's surface, the earth might be supposed to be at rest relative to the ether. But the establishment of the Copernican system made the sun the center of the solar system and gave the earth an orbital speed of eighteen miles a second. It may be remarked parenthetically that the speed of a point on the equator due to the earth's diurnal rotation is quite insignificant compared to its orbital velocity. Hence as a second approximation the sun might be considered at rest relative to the ether and the earth as moving through this unresisting medium.

The first indication of this motion lay in the discovery of aberration by the British astronomer Bradley in 1728. Bradley noticed that stars near the pole of the ecliptic describe small circles during the course of a year, while those in the plane of the ecliptic vibrate back and forth in straight lines, stars in intermediate positions describing ellipses. The surprising thing, however, was that the time taken to complete one of these small orbits is in all cases exactly a year. Bradley concluded that the phenomenon is in some way dependent on the earth's motion around the sun, and he was not long in reaching the correct explanation. For suppose the earth to be at rest. Then in observing a star at the pole of the ecliptic

it would be necessary to keep the axis of the telescope exactly at right angles to the plane of the earth's orbit. However, as the earth is in motion, the telescope must be pointed a little forward, just as in walking rapidly through the rain an umbrella must be inclined forward so as to intercept the raindrops which would otherwise fall on the spot to be occupied at the end of the next step. The angle through which the telescope has to be tilted is known as the angle of aberration, and the tangent of this angle may easily be shown to be equal to the ratio of the velocity of the earth to the velocity of light. Knowing the velocity of the earth, the velocity of light can then be calculated. This method was one of the first of obtaining the value of this important quantity.

More recently, terrestrial methods of great precision have been devised for measuring the velocity of light. The most accurate of these is that employed by the French physicist Foucault in 1862. A ray of light is reflected by a rotating mirror to a fixed mirror placed at some distance, which in turn reflects the ray back to the moving mirror. The latter, however, has turned through a small angle during the time elapsed since the first reflection, and consequently the direction of the ray on returning to the source is not quite opposite to that in which it had started out. This deviation in direction is determined from the displacement of the image formed by the returning light, and from it the velocity of light is calculated. In order to make the deflection appreciable the distance between the two mirrors should be very great. As originally arranged by Foucault, it was found impractical to make this distance greater than twenty meters, and consequently the displacement of the image was less than a millimeter. Such a small deflection limited the accuracy of the experiment to one percent. In 1879, however, Michelson (**18**, 390, 1879), then a master in the United States Navy, improved Foucault's optical arrangements to such an extent that he was able to use a distance of nearly seven hundred meters between the two mirrors. With a rate of two hundred and fifty-seven revolutions a second for the rotating mirror, the displacement obtained was over thirteen centimeters. This experiment gave 299,910 kilometers a second for

the velocity of light, with a probable error of one part in ten thousand. Later investigations by Newcomb and Michelson (**31**, 62, 1886) gave substantially the same result. So great has been the accuracy of these terrestrial determinations that recent practice has been to calculate from them and the angle of aberration the earth's orbital velocity, and hence the distance of the earth from the sun. This indirect method of measuring the astronomical unit has a probable error no greater than the best parallax methods of the astronomer. (J. Lovering, **36**, 161, 1863.)

Aberration is a first order effect, i. e., it depends upon the first power of the ratio of the velocity of the earth to the velocity of light, and at first sight it seemed to prove conclusively that the earth must be in motion relative to the luminiferous medium. Other questions had to be settled, however, and one of these was whether or not light coming from a star would be refracted differently when passing through optical instruments from light which had a terrestial origin. Arago subjected the matter to experiment, and concluded that in every respect the light from a star behaved as if the earth were at rest and the star actually occupied the position which it appears to occupy on account of aberration. Finally optical experiments with terrestrial sources seemed to be in no way affected by the motion of the earth through the ether.

In order to account for these facts Fresnel advanced the following theory. To explain the refraction that takes place when light enters a transparent body, it is necessary to assume that light waves travel more slowly through matter than in free ether. Now the velocity of sound is known to vary inversely with the square root of the density of the material medium through which it passes. Hence it is natural to assume that ether is condensed inside material objects to such an extent that this same relation connects its density with the velocity of light traveling through it. But when a lens or prism is set in motion, Fresnel supposed it to carry along only the *excess* ether which it contains, ether of the normal density remaining behind. This assumption suffices to explain Arago's results, and yet fits in with the phenomenon of aberration. It gives for light traveling in the

direction of motion through a moving material medium of index of refraction n an absolute velocity greater than that when the medium is at rest by an amount

$$\left(1 - \frac{1}{n^2}\right) v,$$

which is only a fraction of the velocity v which would have to be added if convected matter carried along all the ether which resides within it. This expression was tested directly, first by Fizeau in 1851, and later by Michelson and Morley (**31**, 377, 1886) in this country. The experiment consists in bifurcating a beam of light, passing one half in one direction and the other in the opposite direction through a stream of running water. On reuniting the two rays the usual interference fringes are produced. Reversing the direction of motion of the water causes the fringes to shift, and from the amount of this shift the velocity imparted to the light by the motion of the stream is computed. The divergence between the experimental value of this quantity and that calculated from Fresnel's coefficient of entrainment was found by Michelson and Morley to be less than one percent, which was about their experimental error. Thus Fresnel's expression for the velocity of light in a moving medium is entirely confirmed by experiment. The derivation of it accepted to-day, however, is very different from his original deduction.

It has been noted that the phenomena of polarization led Newton to reject the wave theory of light. The only type of wave known to him was the longitudinal wave, in which the vibrations of the particles of the medium are in the same direction as that of propagation of the wave, and it was impossible to suppose that such a wave could have different properties in different directions at right angles to the line in which it is advancing. But in 1817 Young suggested that this inconsistency between the wave theory and the facts of polarization could be removed by supposing the vibrations constituting light to be executed at right angles to the direction of propagation. Thus in ordinary light the vibrations are to be conceived as taking place haphazard in all directions in the plane perpendicular to the ray, while in plane polar-

ized light these vibrations are confined to a single direction. This supposition explained so many of the puzzling results of experiment, that it was accepted at once and led to the complete vindication of the undulatory theory.

Elastic Solid Theory.—Shortly afterwards Poisson succeeded in solving the differential equation which determines the motion of a wave through an elastic medium. His solution shows that such a medium is capable of transmitting two types of wave—one longitudinal, the other transverse. If k denotes the volume elasticity, η the rigidity and ρ the density of the medium, the velocities of the two waves are respectively

$$\sqrt{\frac{k + \frac{4}{3}\eta}{\rho}} \quad \text{and} \quad \sqrt{\frac{\eta}{\rho}}$$

Now a solid has both compressibility and rigidity, and transmits in general both types of wave. A fluid, on the other hand, on account of its lack of rigidity, cannot support a transverse vibration. Hence it was natural that Green, in searching for a dynamical explanation of the ether, should have proposed in a paper read before the Cambridge Philosophical Society in 1837 that the ether has the elastic properties of a solid. One great difficulty presented itself; disturbances inside an elastic solid must give rise to compressional as well as to transverse waves. But no such thing as a compressional wave had been found in the experimental study of light. Green attempted to overcome this difficulty by attributing an infinite volume-elasticity to the ether. The expression above shows that longitudinal waves originating in such an incompressible medium would be carried away with an infinite velocity, and it may be shown that the energy associated with them would be infinitesimal in amount. The next step was to calculate the coefficients of transmission and reflection for light passing from one material medium to another. Here the elastic solid theory is not altogether successful. If the ether is supposed to have different densities in the two media, as in Fresnel's theory, but the same rigidity, certain of these coefficients fail to give the values

demanded by experiment, while if the densities are assumed the same but the rigidities different, other of the coefficients have discordant values. In connection with the phenomena of double refraction even more serious difficulties are encountered.

Electromagnetic Theory.—It was beginning to be felt that an ether must explain more than the phenomena of light, for Faraday's conception of electromagnetic action as carried on through the agency of a medium had added greatly to its functions. Finally Maxwell's demonstration that electromagnetic waves are propagated with the velocity of light made the theory of light into a subdivision of electrodynamics. Maxwell himself did not apply electromagnetic theory to the explanation of reflection and refraction. This deficiency, however, was remedied by Lorentz in 1875. The results obtained, as well as those for double refraction (J. W. Gibbs, **23**, 262, 1882 *et seq.*), and metallic reflection (L. P. Wheeler, **32**, 85, 1911), provided a complete vindication of the electromagnetic theory of light. This is all the more significant when the extreme precision obtainable in optical experiments is taken into account. For instance, Hastings (**35**, 60, 1888) has tested Huygens' construction for double refraction in Iceland spar and found that "the difference between a measured index of refraction . . . at an angle of 30° with the crystalline axis, and the index calculated from Huygens' law and the measured principal indices of refraction" is a matter of only 4·5 units in the sixth decimal place. Since Maxwell's time the gamut of electromagnetic waves has been steadily extended. The shortest Hertzian waves merge almost imperceptibly into the longest heat waves of the infra-red, and from there the known spectrum runs continuously through the visible region to the short waves of the extreme ultra-violet recently disclosed by Lyman. Here there is a short gap until soft X-rays are reached, and finally the domain of radiation comes to an end with gamma rays a billionth of a centimeter in length.

Maxwell's ether was not a dynamical ether in the sense of Green's elastic solid medium. In spite of the fact that Maxwell was always active in devising mechanical ana-

logues to illustrate the phenomena of electromagnetism, he was never enthusiastic over the speculations of the advocates of a dynamical ether. The electrodynamic equations provided an accurate representation of the electric and magnetic fields, and beyond that he felt it was needless to go. That Gibbs (23, 475, 1882) held the same view is made evident by the closing paragraphs of a paper in which he shows that the electromagnetic theory of light accounts in minutest detail for the intricate phenomena accompanying the passage of light through circularly polarizing media. He says:

"The laws of the propagation of light in plane waves, which have thus been derived from the single hypothesis that the disturbance by which light is transmitted consists of solenoidal electrical fluxes, . . . are essentially those which are received as embodying the results of experiment. In no particular, so far as the writer is aware, do they conflict with the results of experiment, or require the aid of auxiliary and forced hypotheses to bring them into harmony therewith.

In this respect the electromagnetic theory of light stands in marked contrast with that theory in which the properties of an elastic solid are attributed to the ether,—a contrast which was very distinct in Maxwell's derivation of Fresnel's laws from electrical principles, but becomes more striking as we follow the subject farther into its details, and take account of the want of absolute homogeneity in the medium, so as to embrace the phenomena of the dispersion of colors and circular and elliptical polarization."

Further Dynamical Theories.—Kelvin, however, was not satisfied with this type of ether. To him dynamics was the foundation of all physical phenomena, and nothing could be said to be explained until a mechanical model was provided. So he returned to the elastic solid theory, and developed the consequences of the assumption, already made use of by Cauchy, that the ether has a negative volume elasticity of such a value as to make the velocity of the compressional wave zero. In order to prevent such an ether from collapsing it is necessary to assume that it is rigidly attached at its boundaries and that cavities cannot be formed at any point in its interior. Now Gibbs (37, 129, 1889) has pointed out the remark-

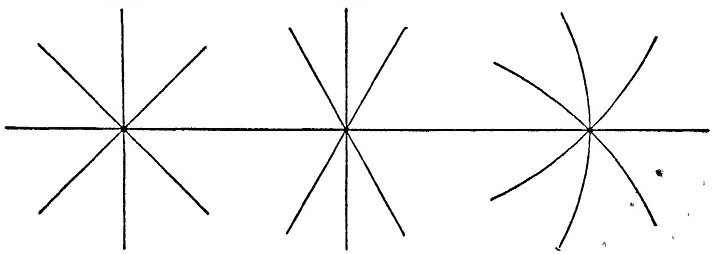
able fact that the equations describing the motion of Kelvin's quasi-labile ether are of exactly the same form as the electromagnetic equations. Electric displacement is represented by an actual displacement of the ether, magnetic intensity by a rotation. Hence everything which can be explained by the electrodynamic equations finds an analogue in terms of Kelvin's ether. Still another type of dynamic ether which fits the known facts was proposed by McCullagh and perfected by Larmor. In this ether a rotational elasticity is premised, such as would exist if each particle of the medium consisted of three rigidly connected gyrostats with mutually perpendicular axes. In this ether electrical displacements correspond to rotations, and magnetic strains to etherial displacement.

A New Point of View.—While the dynamical school was still dominant in England, another point of view was developing on the continent. Kirchhoff denied that it was the province of science to provide mechanical explanations of the ether and electrodynamic phenomena such as Kelvin conceived to be necessary in order to make these phenomena intelligible. Kirchhoff's contention was that the object of science is purely descriptive,—phenomena must be observed, classified, and mutual connections described by the fewest number of differential equations possible. Mach expressed the same idea somewhat more concisely when he asserted that the aim of science is "economy of thought." For instance, in the time of Newton, planetary motions could be described quite satisfactorily by means of the three laws of Kepler. The motion of falling bodies on the earth's surface had been described with a fair degree of accuracy by Galileo. The value of Newton's law of gravitation, however, lay in the fact that this great generalization made it possible to describe these and many other types of motion by a single simple formula, instead of leaving each to be governed by a number of separate and apparently unrelated laws. The importance of such a generalization is measured by the economy of thought which it introduces.

Electron Theory.—The electron theory was leading to a reversal of Kelvin's idea that dynamical principles

must underlie electrodynamics. Lorentz had shown that a rigorous solution of the electrodynamic equations did away entirely with Maxwell's displacement current, but made the electromagnetic field at a point in space depend not upon the distribution of charges and currents at the *same* instant, but at a time earlier sufficient to allow the effect to travel with the velocity of light from the charges and currents producing the field to the point at which the electric and magnetic intensities are to be found. The position of a charge or current element at this earlier time he denoted its "effective position." The effective distribution, then, is that actually *seen* by an observer stationed

FIG. 1. FIG. 2. FIG. 3.



at the point under consideration at the instant for which the intensity of the electromagnetic field is to be determined. This solution of the electrodynamic equations led in turn to rigorous expressions for the electric and magnetic intensities produced by a very small charged particle, such as an electron. Fig. 1 shows the electrostatic field produced by a charged particle at rest. The lines of force spread out radially and uniformly in all directions. In fig. 2 the electron is supposed to have a velocity v horizontally to the right of an amount smaller than, though comparable with, the velocity of light c. It is seen that the lines of electric force still diverge radially from the charge, but are crowded in the equatorial plane and spread apart in the polar regions. The dissymmetry grows as the velocity increases until if the velocity of light should be reached the field would be entirely concentrated in a plane at right angles to the direction of motion. Now it may be shown that fig. 2 is

obtainable from fig. 1 by *reducing dimensions in the direction of motion in the ratio of*

$$\sqrt{1-\beta^2} : 1, \quad \text{where} \quad \beta \equiv \frac{v}{c}.$$

For a uniformly convected electric field differs from an electrostatic field only in that the dimensions in the direction of motion are contracted in this particular ratio. Fig. 3 represents the electric field of a charged particle which has a uniform acceleration to the right. Consider Faraday's analogy between lines of force and stretched elastic bands. The symmetry of the first two figures shows that in neither of these cases would there be a resultant force on the charged particle. But in the third figure it is obvious that a force to the left is exerted on the charge by its own field. Calculation shows this force to be proportional in magnitude to the acceleration. Let it be postulated that the resultant force on a charged particle is always zero. Then if F is the applied force, the force on the particle due to the reaction of its field will be $-mf$, where f stands for the acceleration and m is a positive constant, and we have the fundamental equation of dynamics

$$F - mf = 0$$

Hence, instead of admitting Kelvin's contention that all physical phenomena must be given a mechanical explanation, it would seem more logical to assert that electrodynamics actually underlies mechanics.

Calculation shows the electromagnetic mass m to vary inversely with the radius of the charged particle. Now Thomson's experiments made it possible to calculate the mass of an electron. Hence its radius can be computed, and is found to be about $2(10)^{-13}$ part of a centimeter, or one fifty-thousandth part of the radius of the atom. Since numbers so small convey little meaning, consider the following illustration, due, in part, to Kelvin. Imagine a single drop of water to be magnified until it is as large as the earth. The individual atoms would then have the size of baseballs. Now magnify one of these atoms until it is comparable in size with St. Peter's cathedral at Rome. The electrons within the atom would appear as a few grains of sand scattered about the nave.

This separation between the constituent electrons of the atom,—so great in comparison with their dimensions,—explains how alpha particles can be shot by the billion through thin-walled glass tubing without leaving any holes behind or impairing in the slightest degree the high vacuum within the tube. The much smaller high-speed beta particles pass through an average of ten thousand atoms without even coming near enough to one of the component electrons to detach it and form an ion.

Michelson-Morley Experiment.—In 1881 Michelson (**22**, 120, 1881) conceived an ingenious and bold method of measuring the orbital motion of the earth through the luminiferous ether. As the experiment was one involving considerable expense, Bell, the inventor of the telephone receiver, was appealed to successfully for the funds necessary to carry it through. Michelson's experimental plan was as follows: A beam of light traveling in the direction of the earth's motion strikes an unsilvered mirror m at an angle of 45°. Part of the light passes through, the rest being reflected at right angles to its original direction. Each ray is returned by a mirror at a distance l from m. On meeting again, the ray whose path has been at right angles to the direction of the earth's motion passes on through the mirror, while the other ray is reflected so as to bring the two in line and form interference fringes. Now consider the effect of the earth's motion on the paths of the two rays. In fig. 4 the earth is supposed to be moving to the right. The unsilvered mirror m bifurcates a beam of light coming from a source a. By the time the ray reflected from m has traveled to the mirror b and back, m will have moved forward to m'; a distance $2\beta l$, where the small quantity β is the ratio of the earth's velocity to the velocity of light. Hence the length of the path traversed by this ray is approximately

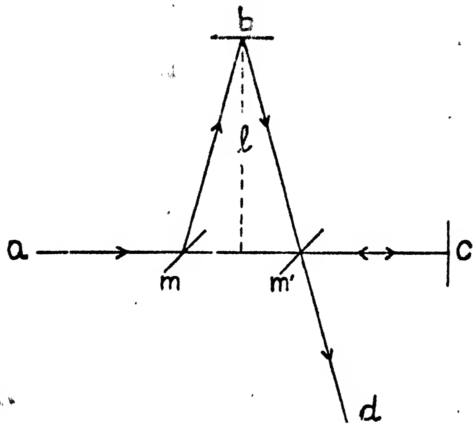
$$2l\left(1+\frac{1}{2}\beta^2\right).$$

The other ray will reach the mirror c after the latter has moved forward a distance

$$\frac{\beta l}{1 - \beta^2},$$

and on returning find m at m'. Hence its path has a length of roughly $2\,l\,(1 + \beta^2)$. The difference in path of the two rays is $\beta^2 l$ and consequently they should be a little out of phase on meeting at d. By rotating the apparatus clockwise through 90° the directions of the two rays relative to the earth's motion are interchanged,

FIG. 4.



and the interference fringes would be expected to shift an amount corresponding to a difference in path of $2\,\beta^2 l$. This quantity is of course small,—β^2 is about one one-hundred millionth,—but so sensitive are the methods of interferometry that Michelson felt confident that he would be able to detect the earth's motion through the ether. The apparatus consisted of a table which could be rotated about a vertical axis in much the same way as a spectrometer table, and provided with arms a meter long to carry the mirrors b and c. With this length of arm the interference fringes from sodium light should shift by an amount corresponding to four hundredths of a wave length when the table is rotated through a right angle. When the experiment was first performed the apparatus was placed on a stone pier in the Physical Insti-

tute at Berlin. So sensitive was the instrument to outside vibrations that even after midnight it was found impossible to get consistent readings. Finally a satisfactory foundation was constructed in the cellar of the Astrophysical observatory at Potsdam. But what was the astonishment of the experimenters to find that the expected shift of the interference fringes did not exist!

The extreme delicacy of the experiment made it desirable to confirm the result by repeating it. This was done by Michelson and Morley (**34**, 333, 1887) in 1887. In place of a revolving table a massive slab of stone floating on mercury was used to carry the apparatus. This slab was kept in constant rotation, the observer following it around. Moreover, the precision of the experiment was greatly increased by reflecting each ray back and forth across the slab a number of times between leaving and returning to the mirror *m*. The accuracy attained was such as to justify Michelson in declaring that if the effect sought actually existed it could not be so great as one-twentieth of its calculated value. In 1905 Morley and Miller[13] repeated the experiment for the second time and succeeded in increasing the sensitiveness of the apparatus to a point such that a motion through the ether of one-tenth of the earth's orbital velocity could have been detected.

The displacement looked for in the Michelson-Morley experiment is known as a second-order effect in that it depends upon the square of the ratio of the velocity of the earth to that of light. Michelson at first considered that the negative result obtained confirmed a theory proposed by Stokes in which it was assumed that the ether inside and near its surface partakes of the motion of the earth, while that at a distance is practically quiescent. But there are many objections to Stokes' theory, one of which was brought out by an experiment of Michelson's (**3**, 475, 1897) in which he attempted by an interference method to detect a difference in the velocity of light at different levels above the earth's surface. The negative result obtained led him to conclude that if Stokes' theory were true the earth's influence on the ether would have to extend to a distance above its surface comparable with its diameter. Meanwhile a more satisfactory explana-

tion was forthcoming. It has been pointed out that a uniformly convected electric field is derivable from an electrostatic field by contracting dimensions in the direction of motion in the ratio

$$\sqrt{1-\beta^2} : 1.$$

Fitzgerald and Lorentz showed independently that if moving matter is distorted in this same way the result obtained by Michelson would be just that to be expected. For then the distance of the mirror c from m would be

$$l\sqrt{1-\beta^2}$$

instead of l, and the path of the ray moving parallel to the earth's orbit

$$2\,l\left(1+\frac{1}{2}\beta^2\right),$$

which is just that of the other ray. Of course when the apparatus is rotated through 90°, the distance of this mirror from m assumes its normal value again, and the distance of the other mirror becomes shortened. As all measurement consists in comparing the object to be measured with a standard this contraction could never be detected by experimental methods, for the measuring rod would contract in exactly the same ratio as the body to be measured.

In computing its electromagnetic mass Abraham had assumed the electron to be a uniformly charged rigid sphere which keeps its spherical form no matter how great a velocity it may be given. He found that the mass increases with the speed at very high velocities, becoming infinite as the velocity of light is approached, and that its value depends upon the direction of the applied force. After the Fitzgerald-Lorentz contraction was seen to be necessary in order to explain Michelson's result, Lorentz calculated the electromagnetic mass of a charged sphere which is deformed into an oblate spheroid when set in motion. For this type of electron too, the mass approaches infinity for velocities as great as that of light, and is different for different directions. If a force is applied in the direction of motion the inertia to

be overcome is a little greater than when the force is applied at right angles to this direction. Thus we have to distinguish between longitudinal and transverse masses. But the masses of Lorentz's electron are not the same functions of its velocity as those of Abraham's. Kaufmann and after him Bucherer tested experimentally the relation between transverse mass and velocity by observing the deflections produced by electric and magnetic fields in the paths of high speed beta particles. The latter's work was such an ample confirmation of Lorentz's formula that it may be considered as proven that a moving electron at least suffers contraction in the direction of motion in the ratio

$$\sqrt{1-\beta^2} : 1.$$

The electromagnetic theory of light had proved so successful when applied to bodies at rest that Lorentz was anxious to extend this theory to the optics of moving media. His problem was to find a group of homogeneous linear transformations that would leave the form of the electrodynamic equations unchanged. The Michelson-Morley experiment had shown that dimensions in the direction of motion must be contracted in the moving system, those at right angles remaining unaltered. But Lorentz soon found that it was also necessary to use a new unit of time in the moving system, and as this time was found to depend upon the *position* of the point at which it is to be determined, he called it the *local* time. Lorentz's transformation is just that of the principle of relativity, but he did not succeed in expressing the electrodynamic equations in terms of the new coördinates and time in exactly the same form as for a system at rest, for the reason that he failed to endow these new units with sufficient reality to justify him in using them when it came to transforming the velocity term involved in an electric current.

Principle of Relativity.—In 1905 appeared in the Annalen der Physik[14] a paper destined to alter entirely the point of view from which problems in light and electromagnetic theory are to be approached. The author was Albert Einstein, of Berne, Switzerland, a young man

of twenty-six who had already made a number of notable contributions to theoretical physics.

The principle of relativity proposed by Einstein was by no means new to students of dynamics. Newton's first two laws of motion express very clearly the fact that in mechanics all motion is relative. Force is proportional to acceleration, and the relation between the two is the same whether the motion under consideration is referred to fixed axes or to axes moving with a constant velocity. But in connection with the phenomena of light and electromagnetism the case seemed to be quite different. There everything was referred to a fixed ether, and even though Lorentz had found a set of transformations which left the electrodymanic equations practically unchanged, he continued to think in terms of an ether. So physicists were not a little startled when Einstein postulated that no experiment, practical or ideal, could ever distinguish between two systems in such a manner as to warrant the assertion that one of them is at rest and the other in motion. All motion is relative, and the laws governing physical, chemical and biological phenomena are the same in terms of the units of one system as in terms of those of any other.

Einstein next considers some very fundamental questions. What do we mean when we say that two events, one at A and the other at a point B far from A, occur at the same time? Obviously the expression has no significance unless synchronous clocks are stationed at the two points. But how is it to be determined whether or not these two clocks are synchronous? If instantaneous communication could be established between A and B the matter would be simple enough. Since no infinite velocity of transmission is available, however, let a light wave be sent from A to B and returned to A immediately upon its arrival. If the time indicated by the clock at B when the signal is received is half way between that at which it left A and the time at which it arrives on its return, then the two clocks may be considered synchronous. Now if it desired to measure the length of a bar which is moving parallel to the scale with which the measurement is to be made, it is necessary to note the positions of the two ends of the bar at the *same* instant.

So even the measurement of the length of a moving body depends upon the condition of synchronism at different points in space.

The principle of relativity requires that the velocity of light shall be the same in one system as in another relative to which the first is in motion. Hence the definition of synchronism makes it possible to obtain a set of transformations connecting space and time measurement on one system with those on another. This group of transformations is exactly that which Lorentz had found would transform the electrodynamic equations into themselves. But Einstein's point of view brought out a remarkable reciprocity which Lorentz had missed. If two parallel rods MN and OP are in motion relative to each other in the direction of their lengths, not only does OP appear shortened to an observer at rest with respect to MN, but MN appears shorter than normal in the same ratio to an observer who is moving along with the rod OP.

Einstein's theory makes the velocity of light the maximum speed with which a signal can be transmitted. This leads to his celebrated addition theorem. Consider three observers A, B and C. Let B be moving relative to A with a velocity of nine-tenths the velocity of light, and C in the same direction with an equal velocity relative to B. In terms of old-fashioned notions of time and space, the velocity of C relative to A would be computed as one and eight-tenths the velocity of light. But the relativity theory gives it as ninety-nine hundredths the velocity of light. For the velocity of light can never be surpassed by that of any material object. This deduction from theory is most strikingly confirmed by the fact that although beta particles have been observed with velocities as high as ninety-nine hundredths that of light, the velocity of light is never quite equalled. It may be remarked in passing that the principle of relativity requires that the masses of all material bodies shall vary with the velocity in the same manner as Lorentz found to be the case for the electromagnetic mass of the deformable electron. In this connection Bumstead (**26**, 498, 1908) has devised an elegant method of deducing the ratio of longitudinal to transverse mass.

The close connection between electrodynamics and the principle of relativity is obvious from the fact that both lead to the same time and space transformations. Furthermore L. Page (37, 169, 1914) has shown that the electrodynamic equations can be derived exactly and in their entirety from nothing more than the kinematics of relativity and the assumption that every element of charge is a center of uniformly diverging lines of force. Hence it may safely be asserted that no purely electromagnetic phenomenon can ever come into contradiction with this principle. The simplicity thus introduced into the solution of a certain class of problems is enormous. As an example consider the question as to whether a moving star is retarded by the reaction of its own radiation. This purely electrodynamical problem is of such complexity that attempts to solve it have led to some controversy among mathematical physicists. The principle of relativity tells us without recourse to analysis that no retardation can exist.

Throughout the nineteenth century the ether has played a fundamental part in all important physical theories of light and electromagnetism. But if it is not possible for experiment to detect even the state of motion of the ether, why postulate the existence of such a medium? If it does not possess the most fundamental characteristic of matter, how can it possess such derived properties as density and elasticity,—properties which any conceivable *mechanical* medium must have in order to transmit transverse vibrations? The relativist does not deny the existence of an ether. To him the question has no more meaning than if he were asked to express an opinion as to the reality of parallels of latitude on the earth's surface. As a convenient medium of expression in describing certain phenomena the ether has justified much of the use which has been made of it. But to attribute to it a degree of substantiality for which there is no warrant in experiment, is to change it from an aid into an obstacle to the progress of science. From the relativist point of view the distinction is very sharp between those motions of charged particles which are experimentally observable, and such geometrical conventions as electromagnetic fields, or analytical symbols as

electric and magnetic intensities. These modes of representation have been and still are of the greatest use and importance, but their value in scientific description must not lead to lack of appreciation of their purely speculative character.

Finally attention must be drawn to the fact that the discoveries of inductive science, embodied in the great generalization we have just been discussing, have led to a more intimate knowledge of the nature of time and space than twenty centuries of introspection on the part of professional philosophers. Minskowski, whose promise of greater achievement was cut off by an untimely death, has shown that four dimensional geometry makes possible the representation with beautiful simplicity of the time and space relationships of this theory. The one time and three space dimensions merge in such a manner as to form a single whole with not a vestige of differentiation between these fundamental quantities. Wilson and Lewis[15] have made this representation familiar to American readers through their admirable translation of Minskowski's work into the notation of Gibbs's vector analysis.

Aberration, the Doppler effect, anomalous dispersion, —indeed all known phenomena,—are found to be in accord with the principle of relativity. It must be borne in mind, however, that this principle applies only to systems moving relative to one another in straight lines with constant velocities. That there is something absolute about rotation has been recognized since Foucault performed his famous pendulum experiment in 1851. This experiment (C. S. Lyman, **12**, 251 and 398, 1851) consisted in setting a pendulum composed of a heavy brass ball suspended by a long wire into oscillation in such a way as to avoid appreciable ellipticity in its motion. Observation of the rate at which the ground rotates relative to the plane of vibration of the pendulum furnished a method of measuring the rotation of the earth about its axis *without reference to celestial bodies.* The gyroscopic compass in use to-day provides yet another terrestrial method of detecting this rotation.

The Future of Physics.—At times during the history of physics it has seemed as if the fundamental laws of

this science had been so completely formulated that nothing remained to future generations beyond the routine of deducing to the full the consequences of these laws, and increasing the precision of the methods used to measure the constants appearing in them. That Laplace held this view has already been pointed out, and Maxwell, in his introductory lecture at the opening of the Cavendish laboratory in 1871, said, "This characteristic of modern experiments—that they consist principally of measurements—is so prominent, that the opinion seems to have gotten abroad that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry on these measurements to another place of decimals." That he himself did not entertain this view is made evident by a succeeding paragraph. "But we have no right to think thus of the unsearchable riches of creation, or of the untried fertility of those fresh minds into which these riches will continue to be poured. It may possibly be true that, in some of those fields of discovery which lie open to such rough observations as can be made without artificial methods, the great explorers of former times have appropriated most of what is valuable, and that the gleanings which remain are sought after rather for their abstruseness than for their intrinsic worth. But the history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers. . . ."

That Maxwell's forecast of the prospects of his science was no overestimate will be granted by those who have followed the progress of physics during the last twenty years. Yet the work accomplished in the past appears small compared to that which is left to the future. Many of the unsolved problems are matters of fitting together puzzling details, but there is at least one whose solution appears to demand a radical modification in our fundamental physical conceptions. This is the formulation of

the laws which govern the motions of electrons and positively charged particles inside the atom.

Black Radiation.—The significance of the problem was first brought to light through the study of black radiation. By a black body is meant one whose distinguishing characteristic is that it emits and absorbs radiation of all frequencies, and black radiation is that which will exist in thermal equilibrium with such a body. The interest of this type of radiation lies in the fact, demonstrated by Kirchhoff, that its nature depends only upon the temperature of the black body with which it is in equilibrium, and on none of this body's physical or chemical characteristics. Thus we may speak of the "temperature" of the radiation itself, meaning by this the temperature of the material body with which it would be in equilibrium.

The problem of black radiation is to find the distribution of energy among the waves of different frequencies at any given temperature. The first step toward a solution was made when Stefan showed experimentally, and Boltzmann as a deduction from thermodynamics and electrodynamics, that the total energy density summed up over all wave lengths varies with the fourth power of the absolute temperature. If the energy density is plotted as ordinate against the wave length as abscissa, the experimental curve for any one temperature rises from the axis of abscissas at the origin, reaches a maximum, and falls to zero again as the wave length becomes infinitely great. Now Wien's displacement law, the second important step toward the determination of the form of this curve, shows that as the temperature is raised the wave length to which its highest point corresponds becomes shorter,—in fact this particular wave length varies inversely with the absolute temperature. This theoretical conclusion is entirely confirmed by experiment. (J. W. Draper, 4, 388, 1847.)

Farther than this general thermodynamical principles are unable to go. Statistical mechanics, however, asserts that when a large number of like elements are in thermal equilibrium, the average kinetic energy associated with each degree of freedom is equal to a universal constant multiplied by the absolute temperature. This "principle of equi-partition of energy" has been applied

in various ways to obtain a radiation law. The most straightforward method is based on the equilibrium which must ensue between radiation field and material oscillators when the latter emit, on the average, as much energy as they absorb. From whatever aspect the problem is treated, however, the radiation law obtained from the application of the equi-partition principle is the same. And while this law agrees well with the experimental curve for long wave lengths, it shows an energy density that becomes indefinitely great for extremely short waves, which is not only at variance with the facts, but actually leads to an *infinite* value of this quantity when integrated over the entire spectrum.

The Energy Quantum.—Now the principle of equipartition of energy rests securely on most general dynamical principles. That these dynamical laws are *inexact* to any such extent as the divergence between theory and experiment would indicate, is inconceivable; that they are *insufficient* when applied to motions of electrons in such intense fields as occur within the atom seems no longer open to doubt. In order to obtain a radiation formula in accord with experiment Planck has found it necessary to extend the atomic idea to energy, which he conceives to exist in multiples of a fundamental quantum $h\nu$, ν being the frequency and h Planck's constant. That some such hypothesis of discontinuity is essential in order to obtain any law that will even approximately fit the experimental facts has been proved by Poincaré. But the precise spot at which the quantum is introduced differs for every new derivation of Planck's law. As deduced most recently by Planck himself, the quantum shows itself in connection with the emission of energy by the material oscillators with which the radiation field is in equilibrium. These oscillators are supposed to act quite normally in every respect except emission; here the radiation demanded by the electrodynamic equations is cast aside, and an oscillator is supposed to emit at once all its energy after it has accumulated an amount equal to some integral multiple of $h\nu$. A form of the theory which does not contain this improbable contradiction of the firmly established facts of electrodynamics introduces the quantum into the specifi-

cation of the energy of vibration which is permitted to each oscillator. Here both emission and absorption follow the classical theory, but the motion of an emitting and absorbing linear oscillator of frequency ν is supposed to be stable only for those amplitudes for which the energy of its oscillations is an integral multiple of $h\nu$. In order to maintain the energy at these particular values, the oscillator may draw energy from, or deposit surplus energy with, other degrees of freedom which partake neither in emission nor absorption, but act merely as storehouses.

Photoelectric Effect.—When investigating the production of electromagnetic waves, Hertz had noticed that a spark passed more readily between the terminals of his oscillator when the negative electrode was illuminated by light from another spark. Further investigation by Hallwachs, Elster and Geitel, and others showed that this effect was due to the emission of electrons by a metal exposed to the influence of ultra-violet light. Lenard discovered that the energy with which a negatively charged particle is ejected is entirely independent of the intensity of the light, and further investigation showed it to depend only on the frequency. Einstein suggested that the electrons appearing in this so-called photo-electric effect start from within the metal with an initial energy $h\nu$. In passing through the surface a resistance is encountered, however, so he concluded that the energy with which the fastest moving electrons appear outside the metal should be equal to $h\nu$ less the work done in overcoming this resistance. Recent experiments not only confirm this relation, but provide a most satisfactory method of determining the value of h. Millikan[16] finds it to be $6{\cdot}57(10)^{-27}$ ergs·sec., which gives the quantum for yellow light a value sixty times as great as the heat energy of a monatomic gas molecule at 0°C. That this large amount of energy can be transferred from the incident light to the ejected electron is quite out of the question; it must come from within the atom. In this way some indication is obtained of how vast intra-atomic energies must be.

Structure of the Atom.—The generally accepted model of the atom is that due chiefly to Rutherford.[17] He con-

siders it to be constituted of electrons revolving about a positive nucleus either singly or grouped in concentric rings, in much the same manner as the planets revolve around the sun. Experiments on the scattering of alpha rays, however, show that the nucleus, while it must have a positive charge sufficient to neutralize the charges of all the electrons moving around it, cannot have a volume of an order of magnitude greater than that of the electron. The number of unit charges residing on it, except in the case of hydrogen, which is supposed to consist of a singly charged nucleus and only one electron, is found to be approximately half the atomic weight. Thus helium, with an atomic weight of about four, has a doubly charged nucleus with two electrons revolving about it, and lithium a triply charged nucleus and three electrons. The number of unit charges on the nucleus is supposed to correspond with the atomic number used by Moseley in interpreting the results of his experiment on the X-ray spectra of the elements.

Now the electron which is revolving around the positive nucleus of a hydrogen atom, must, according to electrodynamic laws, radiate energy. This radiation will act as a resistance to its motion, causing its orbit to become smaller and its frequency to increase. Hence luminous hydrogen would be expected to give off a continuous spectrum. The very fine lines actually found seem inexplicable on the classical dynamical and electrodynamical theories. These lines, and those of many other spectra, may even be grouped into series, and the relations between them expressed in mathematical form. Formulæ have been proposed by Balmer, Rydberg, Ritz and others, all of which contain a universal constant N as well as certain parameters which must be varied by unity in passing from one line of a series to the next.

In 1913 Bohr[18] proposed an atomic theory which brings to light a remarkable numerical relationship between this quantity N and Planck's constant h. He postulated that the electron in the hydrogen atom, for instance, cannot revolve in a circle of any arbitrary radius, but is confined to those orbits for which its kinetic energy is an integral multiple of $\frac{1}{2} h n$, n being its orbital frequency. Now at times this electron is supposed to jump from an

outer to an inner orbit, when the excess energy of the first orbit over the second is radiated away. But the energy emitted is also taken to be equal to $h\nu$, where ν is the frequency of the radiation. Hence ν can be determined, and the expression obtained for it is exactly that given long before by Balmer as an empirical law. The most remarkable thing about it, however, is that Bohr's result contains a constant involving h and the electronic charge and mass which has precisely the value of the universal constant N of Balmer's and Rydberg's formulæ. In all, the theory accounts for three series of hydrogen, and yields satisfactory results for helium atoms which have lost an electron, or lithium atoms which have a double positive charge. But for atoms which retain more than a single electron it seems no longer to hold.

The three mentioned are only the most clearly defined of a growing group of phenomena in which the quantum manifests itself. Its significance and the alteration in our fundamental conceptions to which it seems to be leading is for the future to make clear. That it presents the most important and interesting problem as yet unsolved few physicists would deny.

American Physicists.—In attempting to cover the progress of physics during the last hundred years in the space of a few pages, many important developments the subject have of necessity remained untouched, and the treatment of many others has been entirely inadequate. Among those appearing in the Journal of which no mention has been made are LeConte's (**25**, 62, 1858) discovery of the sensitive flame and Rood's (**46**, 173, 1893) invention of the flicker photometer. However, enough has been recounted to indicate the pre-eminent position in the history of physics in America occupied by four men: Joseph Henry, of the Albany Academy, Princeton, and the Smithsonian Institution; Henry Augustus Rowland, of Johns Hopkins University; Josiah Willard Gibbs, of Yale; and Albert Abraham Michelson, of the United States Naval Academy, Case School of Applied Science, Clark University, and the University of Chicago. Of these, the last named has the distinction of being the only American physicist to have received the Nobel prize, though there is little doubt that

the other three would have been similarly honored had not their important work been published prior to the institution of this award. All four occupy high places in the ranks of the world's great men of science, and the investigations carried out by them and their fellow workers in America have given to their country a position in the annals of physics which is by no means insignificant.

The Journal's Part in Meteorology.

The meteorological investigations published in the early numbers of the Journal have played an important role in establishing a correct theory of storms. Before the origin of the United States Signal Service in 1871 no systematic weather reports were issued by any governmental agency in this country, and consequently the work of collecting as well as interpreting meteorological data rested entirely in the hands of interested individuals and institutions. The earliest important studies of storms to appear in the Journal were contributed by Redfield of New York, whose first paper (**20**, 17, 1831) treated in considerable detail a violent storm which passed over Long Island, Connecticut and Massachusetts in 1821. He concluded that "the direction of the wind at a particular place, forms no part of the essential character of a storm, but is only incidental to that particular portion . . . of the track of the storm which may chance to become the point of observation, . . . the direction of the wind being, in all cases, compounded of both the rotative and progressive velocities of the storm." A few years later, analyses of twelve "gales and hurricanes of the Western Atlantic" (**31**, 115, 1837) led to the statement that the phenomena involved "are to be ascribed mainly to the mechanical gravitation of the atmosphere, as connected with the rotative and orbital movements of the earth's surface." In this paper is emphasized the fact that the wind may blow in diametrically opposite directions at points near the storm center. "While one vessel has been lying-to in a heavy gale of wind, another, not more than thirty leagues distant, has at the very same time been in another gale equally heavy, and lying-to with the wind in quite an opposite direction." From an

accompanying sketch showing wind directions, the reader would infer that, at this time, Redfield believed the motion of the air to be very nearly in circles about the storm center. The same idea is conveyed by a later paper (**42**, 112, 1842). Espy (**39**, 120, 1840) of Philadelphia, however, claimed that observation showed rather that the wind blew inwards toward a central point, if the storm were round in shape, or toward a central line, if it were oblong. This view Redfield (**42**, 112, 1842) contested, and brought forth much evidence to prove its falsity. A later statement (**1**, 1, 1846) of his own theory is as follows: "I have never been able to conceive, that the wind in violent storms moves only in *circles*. On the contrary, a vortical movement . . . appears to be an essential element of their violent and long continued action, of their increased energy towards the center or axis, and of the accompanying rain. . . . The *degree* of vorticular inclination in violent storms must be subject, locally, to great variations; but it is not probable that, on an average of the different sides, it ever comes near to forty-five degrees from the tangent of a circle,—and that such average inclination ever exceeds two points of the compass, may well be doubted." A qualitative explanation of the effect of the earth's rotation on the direction of the wind near the storm center had already been given by Tracy (**45**, 65, 1843), and this was followed some years later by Ferrel's (**31**, 27, 1861) very thorough quantitative investigation of the dynamics of the atmosphere.

A number of individuals kept systematic records of meteorological observations, among whom was Loomis, whose storm analyses did much to settle the merits of the rival theories of Redfield and Espy. In studying the storm of 1836 (**40**, 34, 1841) he had drawn on the map lines through those points in the track of the storm where the barometer, at any given hour, is lowest. While this method revealed the general direction in which the storm was progressing, it failed to give much indication of its size or shape. In discussing the two tornadoes of February, 1842, one of which had already been described in the Journal (**43**, 278, 1842), he adopted a new and more illuminating graphical method. Instead of connect-

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In addition to his work on storms, Loomis made a careful study of the earth's magnetism (**34**, 290, 1838 *et seq.*), and of the aurora borealis (**28**, 385, 1859 *et seq.*). That a connection existed between sunspots, aurora, and terrestrial magnetism was already recognized. Loomis (**50**, 153, 1870 *et seq.*), however, showed that the periodicity of the aurora borealis, as well as of excessive disturbances in the earth's magnetic field, corresponds very closely with that of sunspots.

Notes.

[1] J. W. Gibbs, Trans. Conn. Acad. Arts and Sci., 3, 108 and 343. Abstract by the author, the Journal, 16, 441, 1878.

[2] H. K. Onnes, Nature, 93, 481, 1914.

[3] H. Hertz, Wied. Ann., 34, 551, 1888 *et seq.*

[4] E. F. Nichols and G. F. Hull, Phys. Rev., 13, 307, 1901 *et seq.*

[5] J. J. Thomson, Phil. Mag., 44, 293, 1897.

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[14] 17, 891, 1905.
[15] E. B. Wilson and G. N. Lewis, Proc. Am. Acad. of Arts and Sci., 48, 389, 1912.
[16] R. A. Millikan, Phys. Rev., 7, 355, 1916.
[17] E. Rutherford, Phil. Mag., 21, 669, 1911.
[18] N. Bohr, Phil. Mag., 26, 1, 1913 *et seq.*

XII

A CENTURY OF ZOOLOGY IN AMERICA

By WESLEY R. COE

THIS article is intended as a brief survey of the development of zoology in America, and no attempt is made to give a general history of the science. There are numerous accounts in several languages of zoological history in general, among them being W. A. Locy's "Biology and its Makers." Brief outlines of the history of zoology may be found in many zoological and biological text-books.

For the history of American zoology the reader is referred to Packard's report on "A Century's Progress in American Zoology," published in the American Naturalist, (**10**, 591, 1876), to Packard's "History of Zoology," published in volume **1** of the Standard Natural History (pp. lxii to lxxii, 1885); to G. B. Goode's "Beginnings of Natural History in America,"[1] and "Beginnings of American Science,"[2] and to H. S. Pratt's Manual of the Common Invertebrate Animals (pp. 1-9), 1916. In Binney's "Terrestrial Air-breathing Mollusks of the United States" (1851) is a chapter on the rise of scientific zoology in the United States which well describes the zoological conditions in the early part of the century, while numerous monographs and papers give the history of the investigations on the various groups of animals or on special fields of study.

Brief biographical sketches of the most distinguished of our older Naturalists—Wilson, Audubon, Agassiz, Wyman, Gray, Dana, Baird, Marsh, Cope, Goode and Brooks are given in "Leading American Men of Science," edited by David Starr Jordan, 1910. More extensive biographies have been published separately, and the activities of a number of the more prominent American

zoologists have been recorded in the Biographical Memoirs of the National Academy of Sciences.

The developmental history of zoology in America falls naturally into four fairly well marked periods, namely:—1, *Period of descriptive natural history,* previous to 1847, embracing the early studies on the classification and habits of animals, characteristic of the zoological work previous to the arrival of Louis Agassiz in America. 2, *Period of morphology and embryology,* 1847-1870, during which the influence of Agassiz directed the zoological studies toward problems concerning the relationships of animals as indicated by their structure and developmental history. 3, *Period of evolution,* 1870-1890, when the principle of natural selection received general recognition and the zoological studies were largely devoted to the applications of the theory to all groups of animals. 4, *Period of experimental biology,* since 1890, during which time have occurred the remarkable advances in our knowledge of the nature of organisms through the application of experimental methods in the various branches of the modern science of biology.

American Zoology in 1818.

At the beginning of the century which this volume commemorates, the accumulated biological knowledge of the world consisted mainly of what is to-day called descriptive natural history. The zoölogical treatises of the time were devoted to the names, distinguishing characters and habits of the species of animals and plants known to the naturalists of Europe either as native species or as the results of explorations in other parts of the world. This required little more than a superficial knowledge of their general anatomical structures.

The naturalists of those days had no conception of the life within the cell which we now know to form the basis of all the activities of animals and plants, nor had they even the necessary means of studying such life. The compound microscope, so necessary for the study of even the largest of the cells of the body, was not adapted to such use until 1835, although the instrument was invented in the seventeenth century. With the perfection of the microscope came a period of enthusiastic study of micro-

scopic organisms and microscopic structures of higher animals and plants. It was not until twenty years after the founding of the Journal that the cell theory of structure and function in all organisms was established by the discoveries of Schleiden and Schwann.

The beginning of the nineteenth century saw great zoological activity in Europe, and particularly in France. Buffon's great work on the Natural History of Animals had recently been completed, Cuvier had only one year before published his classic work in comparative anatomy, "Le Regne Animal," and Lamarck's "Philosophie Zoologique" had then aroused a new interest in classification and comparative anatomy from an evolutionary standpoint. É. Geoffroy St.-Hilaire was at the same time supporting an evolutionary theory based on embryonic influences resulting in sudden modifications of adult structure. These epoch-making discoveries and theories gained a considerable following in France, Germany and England, but seem to have had little influence on the zoological work of the following half century in America.

The science of zoology as understood to-day is commonly said to have been founded by Linnæus by the publication of the modern system of classification in the tenth edition of his "Systema Naturæ" in 1758. The influence of Linnæus aroused an interest in biological studies throughout Europe and stimulated new investigations in all groups of organisms. Such studies as related to animals naturally followed first the classification and relationship of species, that is, systematic zoology, and then led gradually into the development of the different branches of the subject, as morphology, comparative anatomy, physiology, and embryology, which eventually were recognized as almost independent sciences.

Of these sciences systematic zoology, which has come to mean the classification, structure, relationship, distribution and habits, or natural history, is the pioneer in any region. Thus we find in our new country at the time of the founding of the Journal in 1818, only sixty years after the publication of Linnæus' great work, the beginning of American zoology taking the form of the collection and description of our native animals.

It is true that many of our more conspicuous and easily collected animals were described long before the opening of the nineteenth century, but this is to be credited mainly to the work of European naturalists who had made expeditions to this country for the purpose of studying and collecting. These collections were then taken to Europe and the results published there. We thus find in the 12th edition of Linnæus descriptions of over 500 American species, about half of which were birds. As an illustration of the extent to which some of these works covered the field even in those early days may be mentioned a monograph in two quarto volumes with many beautifully colored plates on the "Natural History of the rarer Lepidopterous Insects of Georgia." This was published in London in 1797 by J. E. Smith from the notes and drawings of John Abbot, one of the keenest naturalists of any period.

During the early years of the nineteenth century, however, economic conditions in our country became such as to give opportunity for scientific thought. Educated men then formed themselves into societies for the discussion of scientific matters. This naturally led to the establishment of publications whereby the papers presented to the societies could be published and made available to the advancement of science generally. The most influential of these was the Journal of the Philadelphia Academy of Natural Science, which was established in 1817, and was devoted largely to zoological papers. The Annals of the New York Lyceum of Natural History date from 1823, and the Journal of the Boston Society of Natural History from 1834. The Transactions of the American Philosophical Society in Philadelphia and the Memoirs of the American Academy of Arts and Sciences in Boston also published many zoological articles.

In these publications and in the Journal, which was founded in 1818, appear the descriptions of newly discovered animal species, with observations on their habits.

The number of investigators in this field in the first quarter of the nineteenth century was but few, and most of these were compelled to take for the work such time as they could spare from their various occupations.

Gradually the workers became more numerous until

about the middle of the century zoology was taught in all the larger colleges. The science thereby developed into a profession.

For some years the studies remained largely of a systematic nature, and embraced all groups of animals, but long before the close of the century the attention of the majority of the ever increasing group of zoologists was directed into more promising channels for research and there came the development of the sciences of comparative anatomy, physiology, embryology, experimental zoology, cytology, genetics, and the like, while the systematists became specialists in the various animal groups.

But the work in systematic zoology remains incomplete and many native species are still undescribed or imperfectly classified. It is perhaps fortunate that a few faithful systematists remain at their tasks and tend to keep the experimentalists from the disaster which might otherwise result from the confusion of the species under investigation.

Period of Descriptive Natural History.—Previous to 1847.

Of the few American naturalists whose writings were published toward the end of the eighteenth century and at the beginning of the nineteenth the names of William Bartram (1739-1823), Benjamin Barton (1766-1815), Samuel Mitchill (1764-1831), William Peck (1763-1822), and Thomas Jefferson (1743-1826), require special mention. Bartram's entertaining volume describing his travels through the Carolinas, Georgia and Florida, published in 1793, contains a most interesting account of the birds and other animals which he found.

Barton wrote many charming essays on the natural history of animals, but was more particularly interested in botany. Mitchill's most important works include a history of the fishes of New York (1814), and additions to an edition of Bewick's General History of Quadrupeds. The latter, published in 1804, contains descriptions and figures of some American species and is the first American work on mammals.

Peck has the distinction of writing the first paper on systematic zoology published in America. This was a description of new species of fishes and was printed in

1794. He is also well known for his work on insects and fungi.

Jefferson in 1781 published an interesting book describing the natural history of Virginia, and during his presidency was of inestimable service to zoology through his support of scientific expeditions to the western portions of the country.

Previous to Agassiz's introduction of laboratory methods of study in comparative anatomy and embryology in 1847, American naturalists generally confined their attention to the study of the classification and habits of the multitude of undescribed animals and plants of the region.

Such studies were naturally begun on the larger and more generally interesting animals such as the birds and mammals, and although many of these were fairly well described as to species before the opening of the nineteenth century, little was known of their habits. The natural history of our eastern birds first became well known through the accurate illustrations and exquisitely written descriptions of Alexander Wilson (in 1808-1813). Bonaparte's continuation of Wilson's work was published in four folio volumes beginning in 1826.

In 1828 appeared the first of Audubon's magnificent folio illustrations of our birds. These were published in England, with later editions of smaller plates in America. Nuttall's Manual of the Ornithology of the United States appeared in 1832-1834.

The second work on American mammals appeared in the second American edition of Guthrie's Geography, published in 1815. The author is supposed to have been George Ord, although his name does not appear. In 1825 Harlan published his "Fauna Americana: Descriptions of the Mammiferous Animals inhabiting North America." This was largely a compilation from European writers, particularly from Demarest's Mammalogie, and had little value.

In 1826 Amos Eaton published a small "Zoological Text-book comprising Cuvier's four grand divisions of Animals: also Shaw's improved Linnean genera, arranged according to the classes and orders of Cuvier and Latreille. Short descriptions of some of the most

this science had been so completely formulated that nothing remained to future generations beyond the routine of deducing to the full the consequences of these laws, and increasing the precision of the methods used to measure the constants appearing in them. That Laplace held this view has already been pointed out, and Maxwell, in his introductory lecture at the opening of the Cavendish laboratory in 1871, said, "This characteristic of modern experiments—that they consist principally of measurements—is so prominent, that the opinion seems to have gotten abroad that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry on these measurements to another place of decimals." That he himself did not entertain this view is made evident by a succeeding paragraph. "But we have no right to think thus of the unsearchable riches of creation, or of the untried fertility of those fresh minds into which these riches will continue to be poured. It may possibly be true that, in some of those fields of discovery which lie open to such rough observations as can be made without artificial methods, the great explorers of former times have appropriated most of what is valuable, and that the gleanings which remain are sought after rather for their abstruseness than for their intrinsic worth. But the history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers. . . ."

That Maxwell's forecast of the prospects of his science was no overestimate will be granted by those who have followed the progress of physics during the last twenty years. Yet the work accomplished in the past appears small compared to that which is left to the future. Many of the unsolved problems are matters of fitting together puzzling details, but there is at least one whose solution appears to demand a radical modification in our fundamental physical conceptions. This is the formulation of

the laws which govern the motions of electrons and positively charged particles inside the atom.

Black Radiation.—The significance of the problem was first brought to light through the study of black radiation. By a black body is meant one whose distinguishing characteristic is that it emits and absorbs radiation of all frequencies, and black radiation is that which will exist in thermal equilibrium with such a body. The interest of this type of radiation lies in the fact, demonstrated by Kirchhoff, that its nature depends only upon the temperature of the black body with which it is in equilibrium, and on none of this body's physical or chemical characteristics. Thus we may speak of the "temperature" of the radiation itself, meaning by this the temperature of the material body with which it would be in equilibrium.

The problem of black radiation is to find the distribution of energy among the waves of different frequencies at any given temperature. The first step toward a solution was made when Stefan showed experimentally, and Boltzmann as a deduction from thermodynamics and electrodynamics, that the total energy density summed up over all wave lengths varies with the fourth power of the absolute temperature. If the energy density is plotted as ordinate against the wave length as abscissa, the experimental curve for any one temperature rises from the axis of abscissas at the origin, reaches a maximum, and falls to zero again as the wave length becomes infinitely great. Now Wien's displacement law, the second important step toward the determination of the form of this curve, shows that as the temperature is raised the wave length to which its highest point corresponds becomes shorter,—in fact this particular wave length varies inversely with the absolute temperature. This theoretical conclusion is entirely confirmed by experiment. (J. W. Draper, **4**, 388, 1847.)

Farther than this general thermodynamical principles are unable to go. Statistical mechanics, however, asserts that when a large number of like elements are in thermal equilibrium, the average kinetic energy associated with each degree of freedom is equal to a universal constant multiplied by the absolute temperature. This "principle of equi-partition of energy" has been applied

in various ways to obtain a radiation law. The most straightforward method is based on the equilibrium which must ensue between radiation field and material oscillators when the latter emit, on the average, as much energy as they absorb. From whatever aspect the problem is treated, however, the radiation law obtained from the application of the equi-partition principle is the same. And while this law agrees well with the experimental curve for long wave lengths, it shows an energy density that becomes indefinitely great for extremely short waves, which is not only at variance with the facts, but actually leads to an *infinite* value of this quantity when integrated over the entire spectrum.

The Energy Quantum.—Now the principle of equi-partition of energy rests securely on most general dynamical principles. That these dynamical laws are *inexact* to any such extent as the divergence between theory and experiment would indicate, is inconceivable; that they are *insufficient* when applied to motions of electrons in such intense fields as occur within the atom seems no longer open to doubt. In order to obtain a radiation formula in accord with experiment Planck has found it necessary to extend the atomic idea to energy, which he conceives to exist in multiples of a fundamental quantum $h\nu$, ν being the frequency and h Planck's constant. That some such hypothesis of discontinuity is essential in order to obtain any law that will even approximately fit the experimental facts has been proved by Poincaré. But the precise spot at which the quantum is introduced differs for every new derivation of Planck's law. As deduced most recently by Planck himself, the quantum shows itself in connection with the emission of energy by the material oscillators with which the radiation field is in equilibrium. These oscillators are supposed to act quite normally in every respect except emission; here the radiation demanded by the electrodynamic equations is cast aside, and an oscillator is supposed to emit at once all its energy after it has accumulated an amount equal to some integral multiple of $h\nu$. A form of the theory which does not contain this improbable contradiction of the firmly established facts of electrodynamics introduces the quantum into the specifi-

cation of the energy of vibration which is permitted to each oscillator. Here both emission and absorption follow the classical theory, but the motion of an emitting and absorbing linear oscillator of frequency ν is supposed to be stable only for those amplitudes for which the energy of its oscillations is an integral multiple of $h\nu$. In order to maintain the energy at these particular values, the oscillator may draw energy from, or deposit surplus energy with, other degrees of freedom which partake neither in emission nor absorption, but act merely as storehouses.

Photoelectric Effect.—When investigating the production of electromagnetic waves, Hertz had noticed that a spark passed more readily between the terminals of his oscillator when the negative electrode was illuminated by light from another spark. Further investigation by Hallwachs, Elster and Geitel, and others showed that this effect was due to the emission of electrons by a metal exposed to the influence of ultra-violet light. Lenard discovered that the energy with which a negatively charged particle is ejected is entirely independent of the intensity of the light, and further investigation showed it to depend only on the frequency. Einstein suggested that the electrons appearing in this so-called photo-electric effect start from within the metal with an initial energy $h\nu$. In passing through the surface a resistance is encountered, however, so he concluded that the energy with which the fastest moving electrons appear outside the metal should be equal to $h\nu$ less the work done in overcoming this resistance. Recent experiments not only confirm this relation, but provide a most satisfactory method of determining the value of h. Millikan[16] finds it to be $6{\cdot}57(10)^{-27}$ ergs sec., which gives the quantum for yellow light a value sixty times as great as the heat energy of a monatomic gas molecule at 0°C. That this large amount of energy can be transferred from the incident light to the ejected electron is quite out of the question; it must come from within the atom. In this way some indication is obtained of how vast intra-atomic energies must be.

Structure of the Atom.—The generally accepted model of the atom is that due chiefly to Rutherford.[17] He con-

siders it to be constituted of electrons revolving about a positive nucleus either singly or grouped in concentric rings, in much the same manner as the planets revolve around the sun. Experiments on the scattering of alpha rays, however, show that the nucleus, while it must have a positive charge sufficient to neutralize the charges of all the electrons moving around it, cannot have a volume of an order of magnitude greater than that of the electron. The number of unit charges residing on it, except in the case of hydrogen, which is supposed to consist of a singly charged nucleus and only one electron, is found to be approximately half the atomic weight. Thus helium, with an atomic weight of about four, has a doubly charged nucleus with two electrons revolving about it, and lithium a triply charged nucleus and three electrons. The number of unit charges on the nucleus is supposed to correspond with the atomic number used by Moseley in interpreting the results of his experiment on the X-ray spectra of the elements.

Now the electron which is revolving around the positive nucleus of a hydrogen atom, must, according to electrodynamic laws, radiate energy. This radiation will act as a resistance to its motion, causing its orbit to become smaller and its frequency to increase. Hence luminous hydrogen would be expected to give off a continuous spectrum. The very fine lines actually found seem inexplicable on the classical dynamical and electrodynamical theories. These lines, and those of many other spectra, may even be grouped into series, and the relations between them expressed in mathematical form. Formulæ have been proposed by Balmer, Rydberg, Ritz and others, all of which contain a universal constant N as well as certain parameters which must be varied by unity in passing from one line of a series to the next.

In 1913 Bohr[18] proposed an atomic theory which brings to light a remarkable numerical relationship between this quantity N and Planck's constant h. He postulated that the electron in the hydrogen atom, for instance, cannot revolve in a circle of any arbitrary radius, but is confined to those orbits for which its kinetic energy is an integral multiple of $\frac{1}{2} h n$, n being its orbital frequency. Now at times this electron is supposed to jump from an

outer to an inner orbit, when the excess energy of the first orbit over the second is radiated away. But the energy emitted is also taken to be equal to $h\nu$, where ν is the frequency of the radiation. Hence ν can be determined, and the expression obtained for it is exactly that given long before by Balmer as an empirical law. The most remarkable thing about it, however, is that Bohr's result contains a constant involving h and the electronic charge and mass which has precisely the value of the universal constant N of Balmer's and Rydberg's formulæ. In all, the theory accounts for three series of hydrogen, and yields satisfactory results for helium atoms which have lost an electron, or lithium atoms which have a double positive charge. But for atoms which retain more than a single electron it seems no longer to hold.

The three mentioned are only the most clearly defined of a growing group of phenomena in which the quantum manifests itself. Its significance and the alteration in our fundamental conceptions to which it seems to be leading is for the future to make clear. That it presents the most important and interesting problem as yet unsolved few physicists would deny.

American Physicists.—In attempting to cover the progress of physics during the last hundred years in the space of a few pages, many important developments of the subject have of necessity remained untouched, and the treatment of many others has been entirely inadequate. Among those appearing in the Journal of which no mention has been made are LeConte's (25, 62, 1858) discovery of the sensitive flame and Rood's (46, 173, 1893) invention of the flicker photometer. However, enough has been recounted to indicate the pre-eminent position in the history of physics in America occupied by four men: Joseph Henry, of the Albany Academy, Princeton, and the Smithsonian Institution; Henry Augustus Rowland, of Johns Hopkins University; Josiah Willard Gibbs, of Yale; and Albert Abraham Michelson, of the United States Naval Academy, Case School of Applied Science, Clark University, and the University of Chicago. Of these, the last named has the distinction of being the only American physicist to have received the Nobel prize, though there is little doubt that

the other three would have been similarly honored had not their important work been published prior to the institution of this award. All four occupy high places in the ranks of the world's great men of science, and the investigations carried out by them and their fellow workers in America have given to their country a position in the annals of physics which is by no means insignificant.

The Journal's Part in Meteorology.

The meteorological investigations published in the early numbers of the Journal have played an important role in establishing a correct theory of storms. Before the origin of the United States Signal Service in 1871 no systematic weather reports were issued by any governmental agency in this country, and consequently the work of collecting as well as interpreting meteorological data rested entirely in the hands of interested individuals and institutions. The earliest important studies of storms to appear in the Journal were contributed by Redfield of New York, whose first paper (**20**, 17, 1831) treated in considerable detail a violent storm which passed over Long Island, Connecticut and Massachusetts in 1821. He concluded that "the direction of the wind at a particular place, forms no part of the essential character of a storm, but is only incidental to that particular portion . . . of the track of the storm which may chance to become the point of observation, . . . the direction of the wind being, in all cases, compounded of both the rotative and progressive velocities of the storm." A few years later, analyses of twelve "gales and hurricanes of the Western Atlantic" (**31**, 115, 1837) led to the statement that the phenomena involved "are to be ascribed mainly to the mechanical gravitation of the atmosphere, as connected with the rotative and orbital movements of the earth's surface." In this paper is emphasized the fact that the wind may blow in diametrically opposite directions at points near the storm center. "While one vessel has been lying-to in a heavy gale of wind, another, not more than thirty leagues distant, has at the very same time been in another gale equally heavy, and lying-to with the wind in quite an opposite direction." From an

accompanying sketch showing wind directions, the reader would infer that, at this time, Redfield believed the motion of the air to be very nearly *in circles about the* storm center. The same idea is conveyed by a later paper (**42**, 112, 1842). Espy (**39**, 120, 1840) of Philadelphia, however, claimed that observation showed rather that the wind blew inwards toward a central point, if the storm were round in shape, or toward a central line, if it were oblong. This view Redfield (**42**, 112, 1842) contested, and brought forth much evidence to prove its falsity. A later statement (**1**, 1, 1846) of his own theory is as follows: "I have never been able to conceive, that the wind in violent storms moves only in *circles.* On the contrary, a vortical movement . . . appears to be an essential element of their violent and long continued action, of their increased energy towards the center or axis, and of the accompanying rain. . . . The *degree* of vorticular inclination in violent storms must be subject, locally, to great variations; but it is not probable that, on an average of the different sides, it ever comes near to forty-five degrees from the tangent of a circle,—and that such average inclination ever exceeds two points of the compass, may well be doubted." A qualitative explanation of the effect of the earth's rotation on the direction of the wind near the storm center had already been given by Tracy (**45**, 65, 1843), and this was followed some years later by Ferrel's (**31**, 27, 1861) very thorough quantitative investigation of the dynamics of the atmosphere.

A number of individuals kept systematic records of meteorological observations, among whom was Loomis, whose storm analyses did much to settle the merits of the rival theories of Redfield and Espy. In studying the storm of 1836 (**40**, 34, 1841) he had drawn on the map lines through those points in the track of the storm where the barometer, at any given hour, is lowest. While this method revealed the general direction in which the storm was progressing, it failed to give much indication of its size or shape. In discussing the two tornadoes of February, 1842, one of which had already been described in the Journal (**43**, 278, 1842), he adopted a new and more illuminating graphical method. Instead of connect-

ing points of lowest pressure, he drew a curve through all points where the barometer stood at its normal level, then one through those points at which the pressure was 2/10 of an inch below normal, and so on. Temperature he treated in much the same way, and the strength and direction of the wind were indicated by arrows. This innovation gave to his storm analyses a significance which had been entirely lacking in those of his predecessors, and led to the familiar systems of isobars and isotherms in use on the daily charts issued by the Weather Bureau at the present time. Loomis advocated careful observations for one year at stations 50 miles apart all over the United States, so that sufficient data might be obtained to settle once for all the law of storms. His efforts, seconded by those of Henry, Bache, Pierce, Abbe, and Lapham, led eventually to the establishment of the Signal Service, and the publication of daily weather maps according to the plan advocated thirty years before. These maps afforded a basis for further analyses of storms, which he published in numerous "Contributions to Meteorology" (**8**, 1, 1874, *et seq.*) between 1874 and his death in 1890.

In addition to his work on storms, Loomis made a careful study of the earth's magnetism (**34**, 290, 1838 *et seq.*), and of the aurora borealis (**28**, 385, 1859 *et seq.*). That a connection existed between sunspots, aurora, and terrestrial magnetism was already recognized. Loomis (**50**, 153, 1870 *et seq.*), however, showed that the periodicity of the aurora borealis, as well as of excessive disturbances in the earth's magnetic field, corresponds very closely with that of sunspots.

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XII

A CENTURY OF ZOOLOGY IN AMERICA

By WESLEY R. COE

THIS article is intended as a brief survey of the development of zoology in America, and no attempt is made to give a general history of the science. There are numerous accounts in several languages of zoological history in general, among them being W. A. Locy's "Biology and its Makers." Brief outlines of the history of zoology may be found in many zoological and biological text-books.

For the history of American zoology the reader is referred to Packard's report on "A Century's Progress in American Zoology," published in the American Naturalist, (**10**, 591, 1876), to Packard's "History of Zoology," published in volume **1** of the Standard Natural History (pp. lxii to lxxii, 1885); to G. B. Goode's "Beginnings of Natural History in America,"[1] and "Beginnings of American Science,"[2] and to H. S. Pratt's Manual of the Common Invertebrate Animals (pp. 1-9), 1916. In Binney's "Terrestrial Air-breathing Mollusks of the United States" (1851) is a chapter on the rise of scientific zoology in the United States which well describes the zoological conditions in the early part of the century, while numerous monographs and papers give the history of the investigations on the various groups of animals or on special fields of study.

Brief biographical sketches of the most distinguished of our older Naturalists—Wilson, Audubon, Agassiz, Wyman, Gray, Dana, Baird, Marsh, Cope, Goode and Brooks are given in "Leading American Men of Science," edited by David Starr Jordan, 1910. More extensive biographies have been published separately, and the activities of a number of the more prominent American

zoologists have been recorded in the Biographical Memoirs of the National Academy of Sciences.

The developmental history of zoology in America falls naturally into four fairly well marked periods, namely:— 1, *Period of descriptive natural history,* previous to 1847, embracing the early studies on the classification and habits of animals, characteristic of the zoological work previous to the arrival of Louis Agassiz in America. 2, *Period of morphology and embryology,* 1847-1870, during which the influence of Agassiz directed the zoological studies toward problems concerning the relationships of animals as indicated by their structure and developmental history. 3, *Period of evolution,* 1870-1890, when the principle of natural selection received general recognition and the zoological studies were largely devoted to the applications of the theory to all groups of animals. 4, *Period of experimental biology,* since 1890, during which time have occurred the remarkable advances in our knowledge of the nature of organisms through the application of experimental methods in the various branches of the modern science of biology.

American Zoology in 1818.

At the beginning of the century which this volume commemorates, the accumulated biological knowledge of the world consisted mainly of what is to-day called descriptive natural history. The zoological treatises of the time were devoted to the names, distinguishing characters and habits of the species of animals and plants known to the naturalists of Europe either as native species or as the results of explorations in other parts of the world. This required little more than a superficial knowledge of their general anatomical structures.

The naturalists of those days had no conception of the life within the cell which we now know to form the basis of all the activities of animals and plants, nor had they even the necessary means of studying such life. The compound microscope, so necessary for the study of even the largest of the cells of the body, was not adapted to such use until 1835, although the instrument was invented in the seventeenth century. With the perfection of the microscope came a period of enthusiastic study of micro-

scopic organisms and microscopic structures of higher animals and plants. It was not until twenty years after the founding of the Journal that the cell theory of structure and function in all organisms was established by the discoveries of Schleiden and Schwann.

The beginning of the nineteenth century saw great zoological activity in Europe, and particularly in France. Buffon's great work on the Natural History of Animals had recently been completed, Cuvier had only one year before published his classic work in comparative anatomy, "Le Regne Animal," and Lamarck's "Philosophie Zoologique" had then aroused a new interest in classification and comparative anatomy from an evolutionary standpoint. É. Geoffroy St.-Hilaire was at the same time supporting an evolutionary theory based on embryonic influences resulting in sudden modifications of adult structure. These epoch-making discoveries and theories gained a considerable following in France, Germany and England, but seem to have had little influence on the zoological work of the following half century in America.

The science of zoology as understood to-day is commonly said to have been founded by Linnæus by the publication of the modern system of classification in the tenth edition of his "Systema Naturæ" in 1758. The influence of Linnæus aroused an interest in biological studies throughout Europe and stimulated new investigations in all groups of organisms. Such studies as related to animals naturally followed first the classification and relationship of species, that is, systematic zoology, and then led gradually into the development of the different branches of the subject, as morphology, comparative anatomy, physiology, and embryology, which eventually were recognized as almost independent sciences.

Of these sciences systematic zoology, which has come to mean the classification, structure, relationship, distribution and habits, or natural history, is the pioneer in any region. Thus we find in our new country at the time of the founding of the Journal in 1818, only sixty years after the publication of Linnæus' great work, the beginning of American zoology taking the form of the collection and description of our native animals.

It is true that many of our more conspicuous and easily collected animals were described long before the opening of the nineteenth century, but this is to be credited mainly to the work of European naturalists who had made expeditions to this country for the purpose of studying and collecting. These collections were then taken to Europe and the results published there. We thus find in the 12th edition of Linnæus descriptions of over 500 American species, about half of which were birds. As an illustration of the extent to which some of these works covered the field even in those early days may be mentioned a monograph in two quarto volumes with many beautifully colored plates on the "Natural History of the rarer Lepidopterous Insects of Georgia." This was published in London in 1797 by J. E. Smith from the notes and drawings of John Abbot, one of the keenest naturalists of any period.

During the early years of the nineteenth century, however, economic conditions in our country became such as to give opportunity for scientific thought. Educated men then formed themselves into societies for the discussion of scientific matters. This naturally led to the establishment of publications whereby the papers presented to the societies could be published and made available to the advancement of science generally. The most influential of these was the Journal of the Philadelphia Academy of Natural Science, which was established in 1817, and was devoted largely to zoological papers. The Annals of the New York Lyceum of Natural History date from 1823, and the Journal of the Boston Society of Natural History from 1834. The Transactions of the American Philosophical Society in Philadelphia and the Memoirs of the American Academy of Arts and Sciences in Boston also published many zoological articles.

In these publications and in the Journal, which was founded in 1818, appear the descriptions of newly discovered animal species, with observations on their habits.

The number of investigators in this field in the first quarter of the nineteenth century was but few, and most of these were compelled to take for the work such time as they could spare from their various occupations.

Gradually the workers became more numerous until

about the middle of the century zoology was taught in all the larger colleges. The science thereby developed into a profession.

For some years the studies remained largely of a systematic nature, and embraced all groups of animals, but long before the close of the century the attention of the majority of the ever increasing group of zoologists was directed into more promising channels for research and there came the development of the sciences of comparative anatomy, physiology, embryology, experimental zoology, cytology, genetics, and the like, while the systematists became specialists in the various animal groups.

But the work in systematic zoology remains incomplete and many native species are still undescribed or imperfectly classified. It is perhaps fortunate that a few faithful systematists remain at their tasks and tend to keep the experimentalists from the disaster which might otherwise result from the confusion of the species under investigation.

Period of Descriptive Natural History.—Previous to 1847.

Of the few American naturalists whose writings were published toward the end of the eighteenth century and at the beginning of the nineteenth the names of William Bartram (1739-1823), Benjamin Barton (1766-1815), Samuel Mitchill (1764-1831), William Peck (1763-1822), and Thomas Jefferson (1743-1826), require special mention. Bartram's entertaining volume describing his travels through the Carolinas, Georgia and Florida, published in 1793, contains a most interesting account of the birds and other animals which he found.

Barton wrote many charming essays on the natural history of animals, but was more particularly interested in botany. Mitchill's most important works include a history of the fishes of New York (1814), and additions to an edition of Bewick's General History of Quadrupeds. The latter, published in 1804, contains descriptions and figures of some American species and is the first American work on mammals.

Peck has the distinction of writing the first paper on systematic zoology published in America. This was a description of new species of fishes and was printed in

1794. He is also well known for his work on insects and fungi.

Jefferson in 1781 published an interesting book describing the natural history of Virginia, and during his presidency was of inestimable service to zoology through his support of scientific expeditions to the western portions of the country.

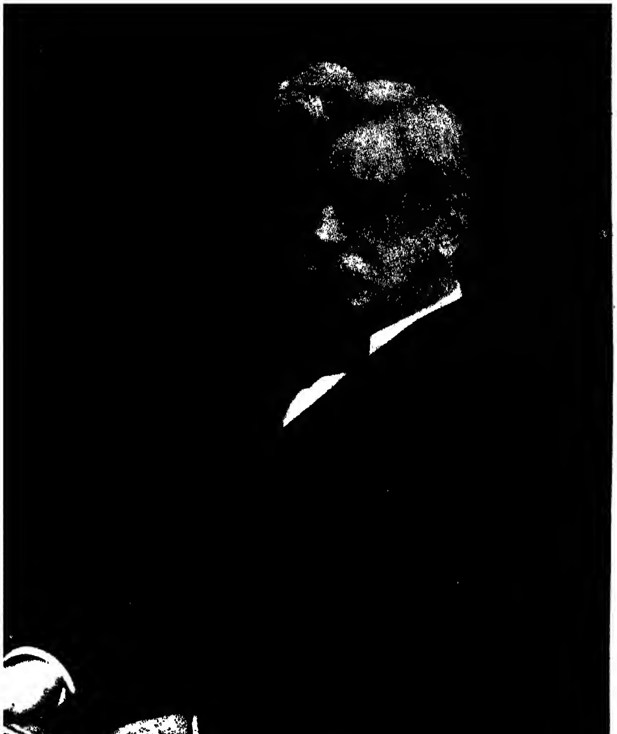
Previous to Agassiz's introduction of laboratory methods of study in comparative anatomy and embryology in 1847, American naturalists generally confined their attention to the study of the classification and habits of the multitude of undescribed animals and plants of the region.

Such studies were naturally begun on the larger and more generally interesting animals such as the birds and mammals, and although many of these were fairly well described as to species before the opening of the nineteenth century, little was known of their habits. The natural history of our eastern birds first became well known through the accurate illustrations and exquisitely written descriptions of Alexander Wilson (in 1808-1813). Bonaparte's continuation of Wilson's work was published in four folio volumes beginning in 1826.

In 1828 appeared the first of Audubon's magnificent folio illustrations of our birds. These were published in England, with later editions of smaller plates in America. Nuttall's Manual of the Ornithology of the United States appeared in 1832-1834.

The second work on American mammals appeared in the second American edition of Guthrie's Geography, published in 1815. The author is supposed to have been George Ord, although his name does not appear. In 1825 Harlan published his "Fauna Americana: Descriptions of the Mammiferous Animals inhabiting North America." This was largely a compilation from European writers, particularly from Demarest's Mammalogie, and had little value.

In 1826 Amos Eaton published a small "Zoological Text-book comprising Cuvier's four grand divisions of Animals: also Shaw's improved Linnean genera, arranged according to the classes and orders of Cuvier and Latreille. Short descriptions of some of the most



A. E. Verrill
Mch. 1904

Zoology in the American Journal of Science, 1870-1918.

The third series of the Journal (1870-1895), likewise including fifty volumes, embraces this period of zoological activity in morphological and embryological studies, culminating with the inception of the modern experimental methods.

In this period also occurred the greatest progress in marine systematic zoology, due to the explorations of the United States Fish Commission off the Atlantic Coast. The Journal had an important share in the zoological development of this period also, for A. E. Verrill, who was now an associate editor, was in charge of the collections of marine invertebrates. Consequently most of the discoveries in this field were published in the Journal in numerous original contributions by Verrill and his associates. The explorations of the U. S. Fish Commission Steamer "Albatross" are described from year to year by Verrill, with descriptions of the new species of invertebrates discovered.

The numerous original contributions by Verrill on subjects of general zoological interest as well as on those of a systematic nature give this third series of the Journal much zoological importance. Verrill's papers cover almost the whole field of descriptive zoology, but are mainly devoted to marine invertebrates. Those which were originally contributed to the Journal or summarized by him in his literature reviews include the following topics:

Sponges, **16**, 406, 1878.

Coelenterates, **37**, 450, 1864; **44**, 125, 1867; **45**, 411, 186, **46**, 143, 1868; **47**, 282, 1869; **48**, 116, 419, 1869; **49**, 370, 1870; **3**, 187, 432, 1872; **6**, 68, 1873; **21**, 508, 1881; **6**, 493, 1898; **7**, 41, 143, 205, 375, 1899; **13**, 75, 1902.

Echinoderms, **44**, 125, 1867; **45**, 417, 1868; **49**, 93, 101, 1870; **2**, 430, 1871; **11**, 416, 1876; **49**, 127, 199, 1895; **28**, 59, 1909; **35**, 477, 1913; **37**, 483, 1914; **38**, 107, 1914; **39**, 684, 1915.

Worms, **50**, 223, 1870; **3**, 126, 1872.

Mollusks, **49**, 217, 1870; **50**, 405, 1870; **3**, 209, 281, 1872; **5**, 465, 1873; **7**, 136, 158, 1874; **9**, 123, 177, 1875; **10**, 213, 1875;

12, 236, 1876; **14**, 425, 1877; **19**, 284, 1880; **20**, 250, 251, 1880; **2**, 74, 91, 1896; **3**, 51, 79, 162, 355, 1897.

Crustacea, **44**, 126, 1867; **48**, 244, 430, 1869; **25**, 119, 534, 1908.

Ascidians, **1**, 54, 93, 211, 288, 443, 1871; **20**, 251, 1880.

Dredging operations and marine fauna, **49**, 129, 1870; **2**, 357, 1871; **5**, 1, 98, 1873; **6**, 435, 1873; **7**, 38, 131, 405, 409, 498, 608, 1874; **9**, 411, 1875; **10**, 36, 196, 1875; **16**, 207, 371, 1878; **17**, 239, 258, 309, 472, 1879; **18**, 52, 468, 1879; **19**, 137, 187, **20**, 390, 1880; **22**, 292, 1881; **23**, 135, 216, 309, 406, 1882; **24**, 360, 477, 1882; **28**, 213, 378, 1884; **29**, 149, 1885.

Miscellaneous, **39**, 221, 1865; **41**, 249, 268, 1866; **44**, 126, 1867; **48**, 92, 1869; **3**, 386, 1872; **7**, 134, 1847; **10**, 364, 1875; **16**, 323, 1878; **20**, 251, 1880; **3**, 132, 135, 1897; **9**, 313, 1900; **12**, 88, 1901; **13**, 327, 1902; **14**, 72, 1902; **15**, 332, 1903; **24**, 179, 1907; **29**, 561, 1910.

S. I. Smith describes the metamorphosis of the crustacea (**3**, 401, 1872; **6**, 67, 1873), species of crustacea (**3**, 373, 1872; **7**, 601, 1874; **9**, 476, 1875), and dredging operations in Lake Superior (**2**, 373, 448, 1871). In this series occurs also a series of papers on comparative anatomy and embryology from the Chesapeake Zoological Laboratory in charge of W. K. Brooks. In the 39th and 40th volumes of the third series (1890) occur several papers on evolutionary topics by John T. Gulick (**39**, 21; **40**, 1, 437) which have attracted much attention.

Before the end of this period, however, the Journal was relieved from the necessity of publishing zoological articles by the establishment of several periodicals devoted especially to the various fields of zoology. We find, therefore, but few exclusively zoological papers after 1885, although articles of a general biological interest and the reviews of zoological books continue.

In the fourth series of the Journal, beginning in 1896, occur also a number of articles on systematic zoology by Verrill and others and several papers having a general biological interest. Brief reviews of a small number of zoological books are still continued, but at the present day the Journal, which played so important a part in the early development of American zoology, has been given over to the geological and physical sciences in harmony with the modern demand for specialization.

Period of Experimental Biology, since 1890.

Zoological studies remained in large measure observational and comparative until about 1890 when the experimental methods of Roux, Driesch and others came into prominence. Interest then turned from the accumulation of facts to an analysis of the underlying principles of biological phenomena. The question now was not so much what the organism does as how it does what is observed, and this question could be answered only by the experimental control of the conditions. These experimental studies met with such remarkable success that in a few years the older morphological studies were largely abandoned, the Morphological Society changed its name to the Society of Zoologists, and in 1904 the Journal of Experimental Zoology was established. The experimental methods were applied to all branches of biological science, and while it must be freely admitted that little progress has been made toward an understanding of the ultimate causes which underlie biological phenomena, a great advance has been made in the elucidation of the general principles involved.

Experimental embryology, histology, regeneration, comparative physiology, neurology, cytology, and heredity have in recent years successfully adopted an experimental aspect and have made significant progress thereby. Biology has now taken its place beside chemistry and physics as an experimental science

The latest great advance in biology has been in the field of heredity. The rediscovery of the Mendelian principles of heredity in 1900 brought to light the most important generalization in biology in recent times. The new science of genetics is essentially the experimental study of heredity.

We are at the moment in the midst of an effort to establish in biology a few relatively simple laws by using for the purpose the vast accumulations of observational data gathered in past years, supplemented by such experimental data as have been provided by these more recent investigations. Such hypotheses as have been formulated are for the most part only tentatively held, for their

validity is generally incapable of a critical test. But wherever such tests have been possible, the laws of mathematics, physics and chemistry are found applicable to biological phenomena.

The number of investigators has now become so great and their activities so prolific that the list and synopses of the zoological publications each year cover upwards of 1000 to 1500 pages in the International Catalogue of Scientific Literature.

American Leadership.—During the first half of the century the progress of zoology in America remained distinctly behind that of Europe. At the beginning of the century the science was farthest developed by the French and English, although Linnæus was a Swede and took his degree in Holland. Under the influence of Von Baer and his monumental treatise on embryology (Ueber Entwicklungsgeschichte der Thiere, 1828), and supported later by the great physiologist, Johannes Müller, whose "Physiologie des Menschen" (1846) forms the basis of modern physiology, the German school forged rapidly ahead and eventually assumed the leadership in zoology, as in several other branches of science.

In the latter half of the century the influence of the German universities dominated in a large measure the zoological investigations in America. The reason for this is partly due to the fact that many of our young zoologists, after finishing their college course, completed their preparation for research by a year or more at a German university. The more mature zoologists, too, looked forward with keen anticipation to spending their summer vacations and sabbatical years in research in a German laboratory or at the famous Naples station in which the German influence was dominant.

With the rise of experimental biology since 1890, however, the American zoologists have shown so high a degree of originality in devising experiments, so much skill in performing them, and such keenness in analyzing the results, that they have assumed the world leadership in several of the special fields into which the science of zoology is now divided.

Biological Periodicals.

Perhaps in no better way can the progress of biology in America be illustrated than by a brief survey of the origin and development of the more important biological journals. For it will be seen that these publications have become more numerous and more specialized as the science has advanced in specialization.

The early publications—which as is well known, treated mainly of the birds, mammals and other vertebrates, and of insects, crustacea and shells—consisted mainly of separate books or pamphlets, published by private subscription. After the establishment of the so-called Academies of Science, or of Arts and Sciences, toward the end of the eighteenth and in the first quarter of the nineteenth century, the reports of the meetings began to be published as periodical Journals, supported by the academies. In these publications, and in the Journal which was founded at the same time, appear papers on all branches of science, including zoology. As soon as zoology in America assumed its modern aspects through the influence of Louis Agassiz and his followers the earliest strictly zoological journals were established.

It should be noted, however, that the journals of the scientific and natural history societies were more or less fully devoted to zoological topics according to the nature of the activities of the members and correspondents. After the establishment of the Museum of Comparative Zoology by Louis Agassiz came the founding in 1863 of its Bulletin and later its Memoirs. These publications have continued to the present day as a standard of excellence for the reports of zoological investigations. In connection with the systematic work on mollusks, the American Journal of Conchology was established in 1865. The American Naturalist was founded in 1867 by four of Louis Agassiz's pupils, Hyatt, Morse, Packard and Putnam. It was later edited by Cope as a leading periodical for the publication of biological papers, particularly those relating to evolution, and is at present devoted to evolutionary topics. It is now in the 52d volume of its new series.

With the awakened interest in comparative anatomy and embryology came the need for an American journal which should supply a means of publication for the reports of researches accomplished by the increasing number of workers in these fields. This need was fully met by the establishment of the Journal of Morphology in 1887. This publication, now in its 30th volume, has equalled the best European journals in the character of its papers. A few years later (1891) came the Journal of Comparative Neurology for the publication of investigations relating to the morphology and physiology of the nervous system and to nervous and allied phenomena in all groups of organisms. Twenty-eight volumes of this journal have been completed. The Zoological Bulletin was started under the auspices of the Marine Biological Laboratory in 1897 for the publication of papers of a less extensive nature and which could be more promptly issued than those in the Journal of Morphology where elaborate plates were required. After two years the scope of the Bulletin was enlarged to include botanical and physiological subjects. The name was correspondingly changed to the Biological Bulletin. Of this important periodical 33 volumes have been issued.

For the publication of papers on human and comparative anatomy and embryology, the American Journal of Anatomy was established in 1901, and is now in its twenty-third volume.

Meanwhile the trend of zoological interest was toward topics connected with the ultimate nature of biological phenomena. The meaning of these phenomena could be determined only by the experimental method. Researches in this field became more prominent and the adequate publication of the numerous papers required the establishment of a new journal in 1904. This was named the Journal of Experimental Zoology. It immediately took its place in the front rank of American zoological periodicals. Twenty-four volumes have been published.

In spite of the constantly increasing number of journals, the science grew faster than the means of publication. So crowded did the American journals become that long delays often resulted before the results of an investigation could be issued. This condition was met in

part by the sending of many papers to be published in European journals (a necessity most discreditable to American zoology) and in part by the establishment ef additional means of publication. Of the latter the Anatomical Record, now in its fourteenth volume, was begun in 1906 for the prompt publication of briefer papers on vertebrate anatomy, embryology and histology and for preliminary reports and notes on technique.

During the past few years has come a great advance in the experimental breeding of plants and animals. Problems in heredity and evolution have taken on a new interest since the importance and validity of Mendel's discovery have been recognized. To meet this development of biology the journal Genetics was begun in 1916 for the publication of technical papers, while the Journal of Heredity, modified from the American Breeders Magazine, is devoted to popular articles on animal and plant breeding, and Eugenics.

On the whole, the science of zoology is now assuming a closer relation to practical affairs. Entomology, for example, is now represented by the Journal of Economic Entomology, of which 10 volumes have been issued since 1907. The Journal of Animal Behavior covers another practical field of research. The Proceedings of the Society for Experimental Biology and Medicine, starting in 1903, the American Journal of Physiology, and several other publications cover the physiological field. The Journal of Parasitology, established 1914, now in its fourth volume, is devoted to the interests of medical zoology. The Auk, now in the 34th volume of its new series (42d of old series), is the official organ of the American Ornithologists Union and is devoted to the dissemination of knowledge concerning bird life. The Annals of the Entomological Society of America, established in 1908, and now in its 10th volume, is one of several important entomological journals. The Nautilus, of which 28 volumes have been issued, is one of the more successful journals devoted to conchology. This list might be extended to include numerous other periodicals of importance, both technical and popular, which have been of great service in the various fields of biology.

In addition to these are the many volumes of syste-

matic papers in the Proceedings of the United States National Museum, the practical reports in the Bulletin of the United States Fish Commission, the vast literature issued yearly by the various divisions of the United States Department of Agriculture, Public Health Service and other Governmental departments, while the list of publications by scientific societies, museums, and other institutes is constantly increasing and covers all fields of biological research.

At the present time facilities for the publication of research on any branch of zoology are as a rule entirely adequate. For this highly satisfactory condition the science is indebted to the support given five of its most important journals by the Wistar Institute of Anatomy and Biology.

Biological Associations.

An important light on the history of biology in America can be thrown by a glance at the rise and development of societies or associations for the report and discussion of papers relating to that branch of science. In the first half of the nineteenth century natural history societies were formed in most cities and centers of learning. These were very important factors in the promotion of scientific research as well as in the diffusion of popular knowledge of living things. The aims and activities of twenty-nine such scientific societies, many of which were devoted especially to natural history, are described in one of the early volumes of the Journal (**10**, 369, 1826). The Connecticut Academy of Arts and Sciences, dating from 1799, the Philadelphia Academy of Natural Sciences from 1812, and the New York Lyceum of Natural History (in 1876 name changed to New York Academy of Sciences) from 1817 are among the oldest of those which still exist.

Of national institutions the American Philosophical Society was founded in 1743, the American Academy of Arts and Sciences in 1780, and the National Academy of Sciences in 1863.

The American Association for the Advancement of Science, with its thousands of members, now has separate sections for each of the special branches of science. This

great association was organized in 1848, as the successor of the Association of American Geologists and Naturalists. This was itself a revival of the American Geological Society which first met at Yale in 1819. Its meetings have given a great support to the scientific work of the country.

The American Society of Naturalists was founded in 1883. The original plan of the society was for the discussion of methods of investigation, administration and instruction in the natural sciences, but its program is now entirely devoted to discussions and papers of a broad biological interest. It also arranges for an annual dinner of the several biological societies and an address on some general biological topic.

In 1890, toward the end of the period in which morphological studies were being emphasized, the professional zoologists of the eastern states founded the American Morphological Society. This association held annual meetings during the Christmas holidays for the presentation of zoological papers. This name became less appropriate after a few years because of the gradual decrease in the proportion of morphological investigations owing to the greater attention being directed to problems in experimental zoology and physiology. Consequently the name was changed to the American Society of Zoologists. To be eligible for membership in this society a person must be an active investigator in some branch of zoology, as indicated by the published results.

The American Association of Anatomists includes in its membership investigators and teachers in comparative anatomy, embryology, and histology as well as in human anatomy. Many professional zoologists and experimental biologists present their papers before this society, or at the meetings of the American Physiological Society. The Entomological Society of America and the American Association of Economic Entomologists are large and active societies.

These national societies have been of great service in fostering a high standard of zoological research. A still more important service, though generally less conspicuous, is rendered by the journal clubs in connection with all the larger zoological laboratories, and by local scien-

tific societies which are now maintained in all the larger centers of learning throughout the country. There are also specific societies for some of the different fields of biological work.

Biological Stations.

No insignificant factor in the development of biological science has been the establishment of biological stations where investigators, teachers and students meet in the Summer vacation for special studies, discussions and research. The most successful of these laboratories have been located on the seashore and here the study of marine life in Summer supplements the work of the school or university biological courses. The famous Naples Station was founded in 1870, and was shortly after followed by several others. Similar biological stations are now supported on almost every coast in Europe and in several inland localities.

The first such American school was established by Louis Agassiz at the island of Penikese on the coast of Massachusetts in 1873, succeeding his private laboratory at Nahant. During that Summer more than forty students gained enthusiasm for the work of future years. Unfortunately the laboratory so auspiciously started was of brief duration, for the death of Agassiz occurred in December of the same year, and the laboratory was discontinued at the end of the following Summer. Shortly afterward Alexander Agassiz equipped a small private laboratory at Newport, Rhode Island, and W. K. Brooks established the Chesapeake Bay Zoological Laboratory.

At this time the United States Fish Commission was engaged under the direction of Spencer F. Baird in a survey of the marine life of the waters off the Eastern Coast. Between 1881 and 1886 the Commission established the splendidly equipped biological station at Woods Hole, Massachusetts. Both here and at the Fish Commission Laboratory at Beaufort, North Carolina, much work in general zoology as well as in economic problems is accomplished. These laboratories are designed particularly for specialists engaged in researches connected with the work of the Fish Commission.

A need was soon felt for a marine laboratory along broader lines, and one available to the students and teachers of the schools and colleges. To meet these requirements the Woods Hole Marine Biological Laboratory was started in 1887, as the successor to an earlier laboratory at Annisquam, and has since become a great Summer congress for biologists from all parts of the country. It is safe to say that no other institution has been of equal service in securing for biology the high plane it now occupies in American science. The leading spirit in the establishment of this laboratory and its director for many years was Charles O. Whitman.

Successful marine laboratories are located also at Cold Spring Harbor, Long Island; at Harpswell, Maine; and at Bermuda. The Carnegie Institution maintains a laboratory at Tortugas Island, Florida, for the investigation of tropical marine life.

On the Pacific Coast marine laboratories are located at Pacific Grove and at La Jolla, California, and at Friday Harbor, Washington. Several other biological laboratories are open each Summer on our coasts, as well as a number of fresh-water laboratories on the interior lakes. There are also several mountain laboratories. The influence of these laboratories on American biology is immeasurable.

Natural History Museums.

Museums of Natural History or "Cabinets of Natural Curios" as they were sometimes called, were established in the first half of the nineteenth century in connection with the various natural history societies. These were of much service in stimulating the collection of zoological "specimens" and in arousing a popular interest in natural history.

The zoological museum of earlier days consisted of rows on rows of systematically arranged specimens, each carefully labelled with scientific name, locality, date of collection and donor—much like the pages of a catalogue. All this has now been changed; the bottles of specimens have been relegated to the storeroom, and the great plate glass cases of the modern museum represent individual studies in the various fields of modern zoological

research, or individual chapters in the latest biological text-books. Often the talent of the artist and the skill of the taxidermist are cunningly combined to produce most realistic bits of nature.

The United States National Museum, the American Museum of Natural History, the Field Columbian Museum and the Museum of Comparative Zoology are among the finest museums of the world, while many of the states, cities, and universities maintain public museums as a part of their educational systems.

Systematic Zoology and Taxonomy.

The work in systematic zoology is now mainly carried on by specialists in relatively small groups of animals. This is necessitated both by the increasingly large number of species known to science and by the completeness and exactness with which species must now be defined. The majority of systematic workers are now connected with museums where the large collections furnish material for comparative studies.

Prominent in this field is the United States National Museum, the publications of which are mainly taxonomic and zoogeographic, and cover every group of organism. The adequacy of this great museum for such studies may be illustrated by the collection of mammals. This museum has the types of 1135 of the 2138 forms (including species and subspecies) of North American mammals recognized in Miller's list,[4] and less than 200 forms lack representatives among the 120,000 specimens of mammals. Systematic monographs of several of the orders of mammals have been published.

Systematic study of the birds has brought the number of species and subspecies known to inhabit North and Middle America to above 3000. The most comprehensive systematic treatise is the still incomplete report of Ridgeway[5] of which seven large volumes have already been issued.

On the reptiles, the most complete monograph is that by Cope[6] entitled "The Crocodilians, Lizards and Snakes of North America."

The Amphibia have also been studied by Cope, whose

report on the Batrachia of North America[7] is the standard taxonomic work.

The most comprehensive systematic work on fishes is the "Descriptive Catalogue of the Fishes of North and Middle America" by Jordan and Evermann.[8]

The invertebrate groups have been in part similarly monographed by the members of the U. S. National Museum staff and others, and further studies are in progress. Other taxonomic monographs published by this museum include the various groups of animals from many different parts of the world.

A number of the larger State, municipal, and university museums publish bulletins on special groups represented in their collections as well as articles of general zoological interest.

Expeditions, subsidized by museum and private funds, are from time to time sent to various parts of the world and their results are often published in sumptuous manner.

The total number of living species of animals is unknown, but considering that about a quarter of a million new species have been described during the past thirty years, it is probable that several million species are in existence to-day. More than half a million have been described. These are probably but a small fraction of the number that have existed in past geological ages.

Thus, in spite of all the work that has been done in systematic zoology and as the number of known species continues to increase, there still remain many groups of animals, some of which are by no means rare or minute, in which probably only a small proportion of the species are as yet capable of identification.

It is only since the publication of Ward and Whipple's "Fresh-water Biology" within the past year that the amateur zoologist could hope to find even the names of all the organisms which may be collected from a single pool of water. And in many cases he will still meet with disappointment, for many of our protozoa and other fresh-water organisms have not yet been described as species.

During the past few years there has been a tendency on

the part of some of our biologists engaged in experimental work to disparage the studies of the systematists. It must be granted, however, that both lines of work are essential to the sound development of zoological science, for experimental investigations in which the accurate diagnosis of species is ignored always result in confusion.

Ecology.—The marvelous modifications in structure and instincts by which the various animals are adapted to their surroundings now forms a special topic in biological research and one of the most fascinating. The adaptations in habitat, time, behavior, appearance and even in structure are found capable of a certain individual modification when studied experimentally.

Zoogeography.—Closely associated with systematic zoology, and indeed a part of the subject in its broader sense, is the study of the geographical distribution of animal species and larger groups.

Paleontology.—The geological succession of organisms embraces a field where zoologist and geologist meet. The wonderful progress made by American investigators is well described in the preceding chapters on Historical Geology and Vertebrate Paleontology.

Biometry.

Since Darwin's theory of evolution postulated the origin of new species by means of natural selection, it was obviously necessary in order to apply a critical test to determine the precise limits of a species. It was, therefore, proposed to subject a given species to a strict examination by the application of statistical methods to determine the range of variation of its members and the extent to which the species intergrades with others. Other problems, particularly those concerning heredity, were treated in similar manner. This branch of biological science was particularly developed by the English School, led by Sir Francis Galton, followed by Karl Pearson and William Bateson.

In America the methods of biometry have been utilized extensively by Charles B. Davenport, Raymond Pearl, H. S. Jennings and others in the solution of problems in genetics and evolution. Their work shows the great

value of critical statistical analysis in the interpretation of biological data. A thorough training in mathematics is now found to be hardly less important for the biologist than is a knowledge of physics and chemistry, for the science of biometry has become one of the most important adjuncts to the study of genetics.

Comparative Anatomy and Embryology.

Comparative Anatomy.—Upon the foundations laid down by Cuvier a century ago the present elaborate structure of comparative anatomy of animals, both vertebrate and invertebrate, has been developed. Vast as is the present accumulation of facts and theories many important problems still await their solution. Jeffries Wyman was long a leader in this field, where many workers are now engaged.

Embryology.—The embryological studies, so brilliantly begun by Von Baer early in the nineteenth century, are still in progress. They have now been extended to the groups more difficult of investigation and into the earliest stages of fertilization and implantation in the mammals. Artificial cultural methods have yielded important results. Louis and Alexander Agassiz, Mark, Minot, Brooks, Whitman, Conklin and E. B. Wilson have taken prominent parts in this work.

In the early nineties embryological studies were directed to the arrangement of cells in the dividing egg, and there was much discussion of "cell lineage" in development. Valuable as were these studies they threw comparatively little light on the general problems of evolution.

Experimental Embryology.—A more fertile field, developed at the same period and a little later, was found in experimental embryology. The discoveries made by Driesch and others in shaking apart the cells of the dividing egg or by destroying one or more of these cells gave a new insight into the potency of cells for compensatory and regenerative processes. These studies attracted many able investigators, who made still further advance by subjecting the germ cells, developing eggs, embryos,

and developing organs to a great variety of artificial conditions.

Artificial Parthenogenesis.—Another question concerns the nature of the process of fertilization and the agencies which cause the fertilized egg to develop into an embryo. In 1899 Jacques Loeb succeeded in causing development in unfertilized sea-urchin eggs by subjecting them to concentrated sea water for a period and then returning them to their normal environment. To this promising field of experimental work came many of the foremost biologists both in America and Europe. It was soon found that the eggs of most groups of animals except the higher vertebrates could be made to develop into more or less perfect embryos and larval forms by treatment with a great variety of chemical substances, by increased temperature, by mechanical stimuli and by other means. This artificial parthenogenesis, as it is called, has also been successful in plants (*Fucus*), and recently Loeb has reared several frogs to sexual maturity by merely puncturing with a sharp needle the eggs from which they were derived. Loeb, then, maintains that "the egg is the future embryo and animal; and that the spermatozoon, aside from its activating effect, only transmits Mendelian characters to the egg."[9]

Further experimental analyses of the nature of the fertilization mechanism have recently been made by Morgan, Conklin, F. R. Lillie, and others.

Germinal Localization.—The question as to whether the egg contains localized organ-forming substances has been studied experimentally particularly by means of the centrifuge. The results indicate that neither of the older opposing theories of "performation" or "epigenesis" is applicable to all eggs, but that in certain organisms the eggs possess a well-marked differentiation while in others each part of the egg is essentially, although probably not absolutely, equipotential.

The Germplasm Cycle.—Since Weismann's postulation of the independence of soma and germplasm in 1885 many attempts have been made to trace the path of the hereditary substance from one generation to the next. A recent book by Hegner[10] summarizes the success attained in various groups of animals.

Cytology.

Another important field of investigation which has attracted many workers is that which pertains to the life of the cell—the science of cytology. Although the cell-theory was established as early as 1839, little advance was made in this subject in America before 1880. Since that time, however, Americans have been so successful in cytological discoveries that they are now among the world's leaders in this field.

These studies have been followed along both descriptive and experimental lines. The most prominent of the early workers in this field are E. L. Mark and E. B. Wilson. Mark's description of the maturation, fecundation, and segmentation of the egg is the most accurate and complete of the early cytological studies. Wilson's discoveries concerning the details of fertilization and his "Atlas of Fertilization and Karyokinesis," published in 1895, have now become classic. Wilson, too, has published the only American text-book on cytology,[11] and has more recently taken the lead in studies concerning the relation between the chromosomes and sex. Besides Wilson, Montgomery, Mark, McClung, Morgan, Miss Stevens, Conklin and their associates and students have now furnished conclusive evidence that the sex of an organism is determined by, or associated with, the nuclear constitution of the fertilized egg. This constitution is moreover shown to be dependent upon the chromosomes received from the germ cells.

This explanation is in strict accordance with the results of experimental breeding. It is also quite in harmony with the Mendelian law of inheritance, and in fact forms one of the strongest supports for the view that all Mendelian factors are resident in the chromosomes. Recent work has also discovered the mechanism which governs the complicated conditions of sex which occur in those animals which exhibit alternating sexual and parthenogenetic generations. These remarkable processes are in all cases found to depend upon a definite distribution of the chromosomes.

Other recent experimental work has shown that while the sex is thus normally determined in the fertilized egg,

it is in some animals not irrevocably fixed, and the normal effect of the sex chromosomes may be inhibited by abnormal conditions in the developing embryo, as is demonstrated by the recent work of Lillie and others.

The cytological basis for Mendelian inheritance has been very extensively studied by Morgan and his pupils in connection with their work on inheritance in the common fruit fly *Drosophila*. The evidence supports Weismann's earlier hypothesis that the chromosomes are the bearers of the heritable factors, and that these are arranged in a series in the different chromosomes. This theory is shown to be in such strict accord with both the cytological studies and the results of experimental breeding that Morgan has ventured to indicate definite points in particular chromosomes as the loci of definite heritable factors, or genes.

Confirmation of this view is furnished by the behavior of the so-called sex-linked characters, the genes for which are situated in the same chromosome as that which carries the sex factor. Many ingenious breeding experiments indicate further that all the hereditary characters in *Drosophila* are borne in four great linkage groups corresponding with the four pairs of chromosomes which the cells of this fly possess.

Comparative Physiology.

None of the experimental fields has been of greater importance in zoological progress than that which concerns the functions of the various organs. Without this companion science morphology and comparative anatomy would have become unintelligible. American investigators, among whom G. H. Parker stands prominent, have taken a leading part in this field also.

Neurology.—The physiological analysis of the components of the nervous system, both in vertebrates and invertebrates, is another important branch of experimental biology. The 28 volumes of the Journal of Comparative Neurology attest the large influence that American investigators have had in the development of this science.

Regeneration.—Experimental studies on the powers of regeneration in plants and animals have been made from the earliest times. During the past few years, how-

ever, there has been made a concerted attempt to analyze the factors which determine the amount and rate of regeneration. Much progress has been made toward the postulation of definite laws applicable to the regenerative processes of the parts of each organism. The critical analyses of Morgan, Loeb and Child have been particularly stimulating.

Tissue Culture.—Another line of experimental work which has been developed within the past few years by Harrison, Carrell, and others is the culture of body tissues in artificial media. These experiments have included the cultivation in tubes or on glass slides of the various tissues of numerous species of animals. They have yielded much information regarding the structure, growth and multiplication of cells, the formation of tissues, and the healing of wounds.

Transplantation and Grafting.—Closely associated experiments consist in the transplantation of organs or other portions of the body to abnormal positions, to the bodies of other animals of the same species or of other species. In this way much has been learned about the potentiality of organs for self-differentiation, for regulation, for regeneration and for compensatory adaptations. The experiments have shown, further, the independence of soma and germplasm and have revealed the nature of certain organs whose functions were previously obscure.

Tropisms and Instincts.—Another field of experimental biology concerns the analysis of behavior of organisms in response to various forms of stimuli. These studies are being prosecuted on all groups of organisms, including the larval stages of many animals, and are yielding most remarkable results. The success in this field of research is largely due to stimulating influence of Jacques Loeb, Parker, Jennings, and their co-workers.

Biological Chemistry.—Still another experimental field which has developed into one of the most important of the biological sciences relates to the fundamental chemical and physical changes which underlie all organic phenomena. A knowledge of both physiological and physical chemistry is to-day essential for all advanced biological work. The peculiar nature of life itself, of growth, disease, old-age, degeneration, death and dissolu-

tion are presumably only manifestations of chemical and physical laws. The ultimate goal of all experimental biology, therefore, will be reached only when the basic physico-chemical properties of life are understood. At that time only will the perennial controversy between vitalism and mechanism be ended.

Economic Zoology.

A moment's reflection will show that economic biology is the most essential of all sciences to the human welfare and progress. For man's relation to his environment is such that the penalty for ignorance or neglect of the biological principles involved in the struggle for existence quickly overwhelms him with a horde of parasites or other enemies.

It is only by the intelligent application of biological knowledge that our food supplies, our forests, our domesticated animals and our bodies can be protected from the ever ravenous organisms which surround us.

The losses to food supplies and other products by insects alone amounts to 100 millions of dollars a month in the United States. And the parasites cause losses in sickness and premature deaths each year of many millions more. Then there are the destructive rodents and other animals which add largely to our burdens of support. These enemies next to wars and fungi are the most destructive agencies on earth. Could they but be eliminated man's struggle against opposing forces would be in large measure overcome. The results of recent work in economic zoology, both in regard to the destruction of enemies and protection of useful mammals, birds and fishes, furnish a bright outlook for the future.

Protozoology.—Partly as an experimental field for the solution of general biological problems and partly because of its practical applications the study of protozoa has now developed into a special science.

The results of the investigations of Calkins, Woodruff, Jennings and others have greatly supplemented our understanding of the signification of such important biological phenomena as reproduction, sexual differentiation, conjugation, tropisms, and metabolism.

From an economic standpoint the protozoa have recently been shown to be of the greatest importance because of the human and animal diseases for which they are responsible.

Parasitology.—The animal parasites of man, domesticated animals and plants include numerous species of protozoa, worms, and insects. Together with the bacteria and a few higher fungi they cause all communicable diseases. When we consider that not only our health but also our entire food supply is dependent upon the elimination of these organisms we must admit that parasitology is the most important economically of all the sciences.

The reports of the investigations of Stiles and his associates in the Hygienic Laboratory and of Ransom and his staff in the Bureau of Animal Industry are widely distributed by the federal government. The systematic studies so ably begun by Joseph Leidy in the middle of the last century have been continued by Ward, Linton, Pratt, Curtis and others on the parasites of many groups of animals.

Economic Entomology.—Another extremely important biological science, the practical applications of which are second only to those of parasitology in importance, is entomology. In the last few years economic entomology has exceeded any of the other branches of biology in the number of its investigators. The American Association of Economic Entomologists has a membership of about five hundred. The work of most of these is supported by appropriations from the State and federal governments, and the results of their investigations are widely published.

It is now well known that some of the protozoon parasites are conveyed from man to man only through the bites of insects. The local eradication of several of our most fatal diseases has recently been brought about by the application of measures to destroy such insects. This is the greatest triumph of economic zoology.

Economic Ichthyology.—The U. S. Fish Commission has for many years been actively engaged in investigations on the food fishes, including methods for increasing

the food supply by suitable protection and artificial propagation. The work includes also edible and otherwise useful mollusks and crustacea. Their marine and fresh-water laboratories have also been of great service to general biological science.

Economic Ornithology and Mammalogy.—In addition to the local bird clubs and the American Ornithologists Union for the study and preservation of bird and mammal life, the Bureau of Biological Survey has for some years conducted investigations on the economic importance of the various species. The publications of this Bureau are of great value both in determining the economic status of our birds and mammals, and also in recommending means for the protection of the beneficial species and the destruction of the injurious. Several of the States issue similar publications.

Genetics.

One of the most interesting chapters in biology relates to the development of the modern science of heredity, or genetics.

Previous to the year 1900, when the Mendelian principle of inheritance was re-discovered, the relative importance of heredity and of environment in the development of an organism was little understood. It is true that Weismann had insisted on the independence of soma and germplasm some years earlier (1883), but the body of the individual was still generally considered the key to its inheritance.

The recognition of the general application of Mendel's discovery gave a great impetus to experimental breeding both in plants and animals. While heretofore it had been necessary to depend upon the somatic characters as evidence of the hereditary constitution of an individual, it now became possible, knowing the hereditary constitution of the parents of any pair of individuals, to predict with almost mathematical certainty the characters of their possible offspring.

In general, the laws of possible chance combinations of any group of characters determine the probability of any particular offspring possessing one or many of those

characters. The physical basis for such Mendelian inheritance is evidently the chance combinations of chromosomes which result from the processes of maturation and union of the germ cells.

Certain limitations to the law are met with because the relatively small number of chromosomes involves linkage of genes, because of the occasional interchange of groups of genes between homologous chromosomes, and because the relative activity or potency of any particular gene may differ in different races, and, finally, because the normal activity of any given gene may be modified or inhibited by the action of other genes. It is by no means certain, however, that all inheritance is Mendelian, for there still remains much evidence that the hereditary basis of certain characters may be resident in the cytoplasm, rather than in the chromosomes. A recent book by Morgan, Sturtevant, Müller and Bridges (1915), entitled "the mechanism of Mendelian heredity" gives the cytological explanation of Mendelian inheritance.

Americans have from the first taken a leading part in this field of research and have been quick to recognize its practical applications to the improvement of breeds in both animals and plants. This prominent position is largely due to the experimental work of Castle, Davenport, Morgan, Jennings, Pearl, and their co-workers on animals and that of East, Emerson, Davis, Hayes and Shull on plants.

The geneticist now realizes that the appearance of the body (phenotype) gives but little clue to the inheritance (genotype). That two white flowers produce only purple offspring, or two white fowls only deeply colored chickens, or that a pair of guinea pigs, one of which is black and the other white, have only gray agouti offspring, while other apparently similar white flowers or white animals produce offspring like themselves, is now readily comprehensible and mathematically predictable.

The most important application of our newly acquired knowledge of inheritance is in the improvement of the human race. The wonderful opportunity in this direction must be apparent to all. The welfare of humanity depends upon the immediate adoption of eugenic princi-

ples. The Eugenics Record Office has secured many of the essential data.

With the destruction of the world's best germ plasm at a rate never equalled before, the outlook for the future race would be appalling were it not for the hope that with the advent of a righteous peace will come a realization of the necessity of applying these new biological discoveries to improving the races of men. That the discoveries have been made too late in the world's history to be of such use to humanity must not be thought possible.

Evolution.

Previous to the publication of Darwin's "Origin of Species" in 1859, American zoologists were generally inclined toward special creation, in spite of the evidences for evolution which had been presented by Erasmus Darwin, Buffon, Lamarck, and Geoffroy St. Hilaire. This attitude of mind continued for some years after the publication of the natural selection theory of Darwin and Wallace. This was in part due to the powerful influence of Louis Agassiz and others who bitterly opposed the Darwinian theory. The influence of Asa Gray in gaining a general acceptance for this theory is explained in the following chapter.

A modified Lamarckian doctrine was widely accepted in the last quarter of the century, due largely to the influence of Cope, Hyatt and Packard. The inheritance of "acquired characters" demanded by this theory seems incompatible with the discoveries of recent times, so that "today the theory has few followers amongst trained investigators, but it still has a popular vogue that is widespread and vociferous."[12]

The origin of new varieties and species by accidental and fortuitous modifications (mutations) of the germ-plasm is now the most widely accepted theory of evolution.

Some of the most important discoveries regarding the origin of new forms have been recently made by Morgan and his pupils. From a stock of the common fruit fly (*Drosophila ampelophila*) more than 125 new types have arisen within six years. Each of these types breeds true. "Each has arisen independently and suddenly. Every

part of the body has been affected by one or another of these mutations." To arrange these mutations arbitrarily into graded series would give the impression of an evolutionary series, but this is directly contrary to the known facts concerning their origin, for each mutation "originated independently from the wild type." "Evolution has taken place by the incorporation into the race of those mutations that are beneficial to the life and reproduction of the individual." This evolutionary process is usually accompanied by the elimination of those forms which have remained stable or which have developed adverse mutations.

A question that is being vigorously debated at this time concerns the possible effects of selection on the hereditary factors. Are the genes fixed both qualitatively and quantitatively or does a given gene vary in potency under different conditions and in different individuals? In the former case selection can only separate the existing genes into separate pure strains. But if the gene be quantitatively variable, then selection will result in the establishment of new types.

Castle has long stoutly maintained the effect of such selection, and his forces have recently been augmented by Jennings. The experimental work now in process will doubtless yield a decisive answer.

Conclusion.

A comparison of the simple descriptive natural history of a century ago with the foregoing manifold developments of modern biology will indicate the wonderful progress which has occurred during this period. The path has led from the crude methods of the almost unaided eye and hand to the applications of the most delicate experimental apparatus. For the marvelous success which zoology has attained has been possible only by the skillful use of scalpel, microscope, microtome and other mechanical devices and by the refined methods of the chemist and physicist.

The central truth to which all these discoveries consistently point is the unity and harmony of all biological phenomena, and indeed of all nature. No longer does the zoologist find any demarcated line separating his field of

research from that of the botanist or the chemist or even of the physicist; for all the natural sciences obviously deal with closely associated phenomena. The aim of the future will be both to complete fields of study already marked out and to derive a comprehensive explanation of the general principles involved.

Notes.

[1] Proc. Biol. Soc. Washington, 3, 35, 1886.

[2] Ibid, 4, 9, 1888. Both of these papers are reprinted in Ann. Rept. Smithsonian Inst., 1897, U. S. Nat. Mus., Pt. 2, pp. 357-466, 1901.

[3] Louis Agassiz: his Life and Correspondence, by Elizabeth Carey Agassiz, p. 145, 1885.

[4] List of North American Land Mammals in the United States National Museum, 1911. Bull. 79, U. S. Nat. Mus., 1912.

[5] Birds of North and Middle America, Bull. 50, parts I-VII, U. S. Nat. Mus., 1901-1916.

[6] Report U. S. Nat. Mus. for 1898, pp. 153-1270, 1900.

[7] Bull. 34, U. S. Nat. Mus., 1889.

[8] Bull. 47, parts I-IV, U. S. Nat. Mus., 1896-1900.

[9] J. Loeb, The Organism as a Whole, p. 126, 1916.

[10] The Germ-cell Cycle in Animals, 1914.

[11] The Cell in Development and Inheritance, 1896; second edition, 1900.

[12] Morgan, T. H. A critique of the theory of evolution, p. 32, 1916.

XIII

THE DEVELOPMENT OF BOTANY SINCE 1818

By GEORGE L. GOODALE

> *"Our Botany, it is true, has been extensively and successfully investigated, but this field is still rich, and rewards every new research with some interesting discovery."*

SUCH are the words with which the sagacious and far-sighted founder of the American Journal of Science and Arts, in his general introduction to the first volume, alludes to the study of plants. It is plain that the editor, embarking on this new enterprise, appreciated the attractions of this inviting field and sympathetically recognized the good work which was being done in it. It is not surprising, therefore, to find that he welcomed to the pages of his initial number contributions to botany.

Early Botanical Works.—The collections of dried and living North American plants, which had been carried from time to time to botanists in Europe, had been eagerly studied, and the results had been published in accessible treatises. Besides these general treatises, there had been issued certain works, wholly devoted to the American Flora. Among these latter may be mentioned Pursh's "Flora" (1814) and Nuttall's "Genera" (1818). There were also a few works which were rather popular in their character, such as Amos Eaton's "Manual of Botany for North America" (1817), and Bigelow's "Collection of the Plants of Boston and environs" (1814). These handbooks were convenient, and possessed the charm of not being exhaustive; consequently a botanist, whether professional or amateur, was stimulated to feel that he had a good chance of enriching the list of species and adding to the next edition.

The Early Years of Botany in the Journal.

At that time, the botanists had no journal in this country devoted to their science. Here and there they found opportunity for publishing their discoveries in some medical periodical or in a local newspaper. Hence American botanists availed themselves of the welcome extended by Silliman to botanical contributors to place their results on record in a magazine devoted to science in its wide sense. Specialization and subdivision of science had not then begun to dissociate allied subjects, and, consequently, botanists felt that they would be at home in this journal conducted by a chemist. Botanists responded promptly to this invitation with interesting contributions.

It is well to remember that the appliances at the command of naturalists at the date when the Journal began its service, were imperfect and inadequate. The botanist did not possess a convenient achromatic microscope, and he was not in possession of the chemical aids now deemed necessary in even the simplest research. Hence, attention was given almost wholly to such matters as the forms of plants and the more obvious phenomena of plant-life. In view of the poverty of instrumental aids in research, the results attained must be regarded as surprising.

In the very first volume of the Journal, bearing the date of 1818, there are descriptions of four new genera and of four new species of plants; certainly a large share to give to systematic botany. Besides these articles, there are some instructive notes concerning a few plants, which up to that time had been imperfectly understood. There are four Floral Calendars which give details in regard to the blossoming and the fruiting of plants in limited districts, a botanical subject of some importance but likely to become tedious in the long run. Just here, the skill of the editor in limiting undesirable contributions is shown by his tactful remark designed to soothe the feelings of a prolix writer whose too long list of plants in a floral calendar he had editorially cut down to reasonable limits. The editor remarks, "such extended observations are desirable, but it may not always be convenient

to insert very voluminous details of daily floral occurrence.'' It is convenient to consider by themselves some of the botanical contributions published in the first series of volumes of the Journal during a period of twenty years, the period before Asa Gray became actively and constantly associated with the Journal.

In systematic and geographical botany one finds communications from Douglass and Torrey (**4**, 56, 1822) on the plants of what was then the North-west; Lewis C. Beck (**10**, 257, 1826; **11**, 167, 1826; **14**, 112, 1828) contributed valuable papers on the botany of Illinois and Missouri; there is a literal translation by Dr. Ruschenberger (**19**, 63, 299, 1831; **20**, 248, 1831; **23**, 78, 250, 1833) of a very long list of the plants of Chili; Wolle and Huebener (**37**, 310, 1839) gave an annotated catalogue of botanical specimens collected in Pennsylvania; Tuckerman (**45**, 27, 1843) presented communications in regard to numerous species which he had examined critically; Darlington (**41**, 365, 1841) published his lecture on grasses; Asa Gray (**40**, 1, 1841) gave an instructive account of European herbaria visited by him, and he contributed also a charming account (**42**, 1, 1842) of a botanical journey to the mountains of North Carolina. The most extensive series of botanical communication at this time was the Caricography by Professor Dewey of Williams College, presented in many numbers of the Journal; the first of these in **7**, pp. 264-278, 1824. There were also descriptions of certain new genera, and species, and critical studies in synonyms.

Cryptogamic botany is represented in the first series of volumes of the Journal by L. C. Beck's (**15**, 287, 1829) study of ferns and mosses, by Bailey's (**35**, 113, 1839) histology of the vascular system of ferns, by Fries' Systema mycologicum (**12**, 235, 1829), and by De Schweinitz (**9**, 397, 1825) and Halsey, who had in hand a cryptogamic manual. There are two important papers by Alexander Braun, translated by Dr. George Engelmann, one on the Equisetaceæ of North America (**46**, 81, 1844) and the other on the Characeæ (**46**, 92, 1844).

Vegetable paleontology had begun to attract attention in many places in this country, and therefore the translated contributions by Brongniart on fossil plants were

given space in the Journal. Plant-physiology received a good share of attention either in short notices or in longer articles. Such titles appear as, the respiration of plants, the circulation of sap, the excrementitious matter thrown off by plants, the effects of certain gases and poisons on plants, and the relations of plants to different colored light. One of the most important of the notes is that in which is described the discovery by Robert Brown (**19**, 393, 1831) of the constant movement of minute particles suspended in a liquid, first detected by him in the fovilla of pollen grains, and now known as the Brownian (or Brunonian) movement. The heading under which this note appears is of interest, "The motion of living particles in all kinds of matter."

One side of botany touches agriculture and economics. That side was represented even in the first volume of the Journal by a study of "the comparative quantity of nutritious matter which may be obtained from an acre of land when cultivated with potatoes or wheat." Succeeding volumes in this series likewise present phases which are of special interest regarded from the point of view of economics; for example, those which treat of rotation of crops and of enriching the soil. Probably the economic paper which may be regarded as the most important, in fact epoch-making, is the full account of the invention by Appert of a method for preserving food indefinitely (**13**, 163, 1828). We all know that Appert's process has revolutionized the preservation of foods, and in its modern modification underlies the vast industry of canned fruits, vegetables and so on. There are suggestions, also, as to the utilization of new foods, or of old foods in a new way, which resemble the suggestions made in these days of food conservation. For example, it is shown that flour can be made from leguminous seeds by steaming and subsequent drying, and pulverizing. There are excellent hints as to the best ways of preparing and using potatoes, and also for preserving them underground, where they will remain good for a year or two. It is shown that potato flour can be made into excellent bread. Another method of making bread, namely from wood, is described, but it does not seem quite so practicable.

There are interesting notes on the sugar-beet as a source of sugar, and here appears one of the earliest accounts of the Assam tea-plant, which was destined to revolutionize the tea industry throughout the world. Cordage and textile fibers of bark and of wood should be utilized in the manufacture of paper. In fact one comes upon many such surprises in economic botany as the earlier volumes of the Journal are carefully examined.

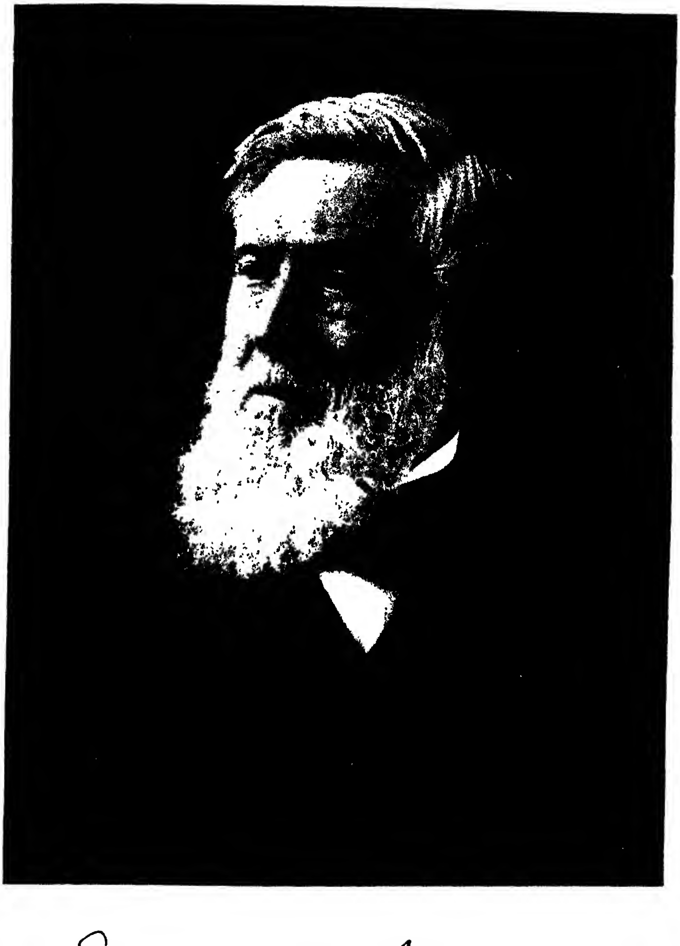
Early numbers of the Journal present with sufficient fulness accounts of the remarkable discovery by Daguerre and others of a process for taking pictures by light, on a silver plate or upon paper (**37**, 374, 1839; **38**, 97, 1840, etc.). Before many years passed, the Journal had occasion to show that these novel photographic delineations could be made useful in the investigation of problems in botany. In the pages of the Journal it would be easily possible to trace the development of this art in its relations to natural history. Silliman possessed great sagacity in selecting for his enterprise all the novelties which promised to be of service in the advancement of science. In 1825 (**9**, 263) the Journal republished from the Edinburgh Journal of Science an essay by Dr. (afterwards Sir) William Jackson Hooker, on American Botany. In this essay the author states that "the various scientific Journals" which "are published in America, contain many memoirs upon the indigenous plants. Among the first of these in point of value, and we think also the first with regard to time, we must name Silliman's Journal of Science." The author enumerates some of the contributors to the Journal and the titles of their papers.

It has been a useful practice of the Journal, almost from the first, to transfer to its pages memoirs which would otherwise be likely to escape the notice of the majority of American botanists. The book notices and the longer book reviews covered so wide a field that they placed the readers of the Journal in touch with nearly all of the current botanical literature both here and abroad. These critical notices did much towards the symmetrical development of botany in the United States. And as we shall now see, the Journal notices and reviews in the

hands of Asa Gray continued to be one of the most important factors in the advancement of American botany.

Asa Gray and the Journal.

In 1834 there appears in the Journal (25, 346) a "Sketch of the Mineralogy of a portion of Jefferson and St. Lawrence Counties, New York, by J. B. Crawe of Watertown and A. Gray of Utica, New York." This appears to be the first mention in the Journal of the name of Dr. Asa Gray, who, shortly after that date, became thoroughly identified with its botanical interests. In the early part of his career both before and immediately after graduating in medicine, Gray gave much attention to the different branches of natural history in its wide sense. He not only studied but taught "chemistry, geology, mineralogy, and botany," the latter branch being the one to which he devoted most of his attention. Among his early guides in the pursuit of botany may be mentioned Dr. Hadley, "who had learned some botany from Dr. Ives of New Haven," and Dr. Lewis C. Beck of Albany, author of Botany of the United States North of Virginia. At that period he made the acquaintance of Dr. John Torrey of New York, with whom he later became associated in most important descriptive work. During the years between his graduation in medicine and 1842, the year when he came to Harvard College, his activities were diverse and intense; so that his preparation for his distinguished career was very broad and thorough. His first visit to Europe, in 1838, brought him into personal relations with a large number of the botanists of Great Britain and the Continent. This extensive acquaintance, added to his broad training, enabled him even from the outset to exert a profound influence upon the progress of his favorite science. He made the Journal tributary to this development. His name first appears as associate editor in 1853, but there are articles in the Journal from his pen which bear an earlier date. The first of these early botanical papers is the following: "A Translation of a memoir entitled 'Beiträge zur Lehre von der Befruchtung der Pflanzen,' (contributions to the doctrine of the impregnation of plants, by A. J. C.



Sincerely Yours

Asa Gray

Corda:) with prefatory remarks on the progress of discovery relative to vegetable fecundation; by Asa Gray, M. D." (31, 308, 1837). Dr. Gray says that he made the translation from the German for his own private use, but thinking that it might be interesting to the Lyceum, he brought it before the Society, with "a cursory account of the progress of discovery respecting the fecundation of flowering plants, for the purpose of rendering the memoir more generally intelligible to those who are not particularly conversant with the present state of botanical science." The translation occupies six pages of the Journal, while the prefatory remarks fill nine pages. The prefatory remarks constitute an exhaustive essay on the subject, embodied in attractive and perfectly clear language. The translator shows complete familiarity with the matter in hand and gives an adequate account of all the work done on the subject up to the date of M. Corda's paper. A second important paper by him near this period is his review of "A Natural System of Botany: or a systematic view of the Organization, Natural Affinities, and Geographical Distribution of the whole Vegetable Kingdom; together with the use of the more important species in Medicine, the Arts, and rural and domestic economy, by John Lindley. Second edition, with numerous additions and corrections, and a complete list of genera and their synonyms. London: 1836" (32, 292, 1837). A very brief notice of this work in the first part of the volume for 1837 closes with the words, "A more extended notice of the work may be expected in the ensuing number of the Journal." The extended notice proved to be a critical study of the work, signed by the initials A. G. which later became so familiar to readers of the Journal. Citation of a few of its sentences will indicate the strong and quiet manner in which Dr. Gray, even at the outset, wrote his notices of books. In speaking of the second edition of Professor Lindley's work, he says:

"It is not necessary to state that a treatise of this kind was greatly needed, or to allude to the peculiar qualifications of the learned and industrious author for the accomplishment of the task, or the high estimation in which the work is held in Europe. But we may properly offer our testimony respecting the great

and favorable influence which it has exerted upon the progress of botanical science in the United States. Great as the merits of the work undoubtedly are, we must nevertheless be excused from adopting the terms of extravagant and sometimes equivocal eulogy employed by a popular author, who gravely informs his readers that no book, since printed Bibles were first sold in Paris by Dr. Faustus, ever excited so much surprise and wonder as did Dr. Torrey's edition of Lindley's Introduction to the Natural System of Botany. Now we can hardly believe that either the author or the American editor of the work referred to was ever in danger, as was honest Dr. Faustus, of being burned for witchcraft, neither do we find anything in its pages calculated to produce such astonishing effects, except, perhaps, upon the minds of those botanists, if such they may be called, who had never dreamed of any important changes in the science since the appearance of good Dr. Turton's translation of the Species Plantarum, and who speak of Jussieu as a writer who has greatly improved the natural orders of Linnæus."

In the Journal for 1840 there is a large group of unsigned book reviews under the heading, "Brief notices of recent Botanical works, especially those most interesting to the student of North American Botany." The first of these short reviews deals with the second section of Part VII of De Candolle's "Prodromus." In 1847 the consideration of the "Prodromus" is resumed by the same author and the initials of A. G. are appended. This indicates that Dr. Gray was probably the writer of some of the unsigned book-reviews which had appeared in the Journal between 1837 and 1840. Doubtless Silliman availed himself of the assistance of his associates, Eli Ives and others, in New Haven, in the examination of current botanical literature, and it is extremely probable that he early secured help from young Dr. Gray, who had shown himself to be a keen critic as well as a pleasing writer. The notices of botanical works from 1840 bear marks of having been from the same hand. They cover an extremely wide range of subjects. While they are good-tempered they are critical, and they had much to do with the development of botany, in this country, along safe lines.

Gray as Editor.—Gray's name as associate editor of the Journal appears in 1853. He had been a welcome contributor, as we have seen, for many years. His

influence upon the progress of botany in the United States was largely due to his connection with the Journal. His reviews extended over a very wide range, and supplemented to a remarkable degree his other educational work. It must be permitted to allude here to his sagacity as a writer of educational treatises. In his first elementary text-book, published in 1836, he expressed wholly original views in regard to certain phases of structure and function in plants, which became generally adopted at a later date. His Manual of Botany was constructed, and subsequent editions were kept, on a plan which made no appeal to those who wanted to work on lines of least resistance; in fact he had no patience with those who desired merely to ascertain the name of a plant. In the Journal he emphasizes the desirability of learning all the affinities of the plant under consideration. At a later period, when entirely new chapters had been opened in the life of plants, he sought by his contributions in the Journal to interest students in this wider outlook.

Professor C. S. Sargent has selected with good judgment some of the more important scientific papers by Professor Gray and has re-published them in a convenient form.[1] Many of these papers were contributed to the Journal in the form of reviews. These reviews touch nearly every branch of the science of botany. As Sargent justly says, "Many of the reviews are filled with original and suggestive observations, and taken together, furnish the best account of the development of botanical literature during the last fifty years that has yet been written." In these longer reviews in the Journal, Gray was wont to take a book under review as affording an opportunity to illustrate some important subject, and many of the reviews are crowded with his expositions. For example, in his examination of vonMohl's "Vegetable Cell" (15, 451, 1853) he takes up the whole subject of microscopic structure, so far as it was then understood, and he points out the probable errors of some of Mohl's contemporaries, showing what and how great were Mohl's own contributions to histology. Such a review is a landmark in the science. The physiology of the cell and the nutrition of the plant were favorite topics with Professor Gray, and he brought

much of his knowledge in regard to them into such a review as that of Boussingault (**25**, 120, 1858) on the "Influence of nitrates on the production of vegetable matter."

As a systematic botanist, Gray was naturally much interested in the vexed question of nomenclature of plants. One of his most important communications to the Journal is his review, in the volume for 1883 (**26**, 417), of DeCandolle's work on the subject. He deals with this strictly technical matter much as he did in a contribution to the Journal which he made in 1868 (**46**, 63). In both of these papers he states with clearness the general features of the code of nomenclature. He says explicitly that the code does not make, but rather declares, the common law of botanists. The treatment of the subject at his hands would rightly impress a general reader as showing a strong desire to have common sense applied to doubtful cases, instead of insisting on inflexible rules. For this reason, his rule of practice was not always acceptable to those who were anxious to secure conformity to arbitrary rules at whatever cost. As he said in a paper published in the Journal in 1847 (**3**, 302), "The difficulty of a reform increases with its necessity. It is much easier to state the evils than to relieve them; and the well-meant endeavors that have recently been made to this end, are, some of them, likely, if adopted, to make confusion worse confounded." This feeling led him to be very conservative in the matter of reform in nomenclature.

This subject of botanical nomenclature illustrates a method frequently employed by Professor Gray to elucidate a difficult matter. He would find in the treatise under review a text, or texts, on which he would build a treatise of his own, and in this way he made clear his own views relative to most of the important phases of botany. When he faced controverted matters, his attitude still remained judicial. While he was tolerant of opinions which clashed with his own, he was always severe upon charlatanism and impatient of inaccuracy. The pages of the Journal contain many severe criticisms at his hands, but an unprejudiced person would say that the severity is merited.

Sometimes, however, instead of reviewing a book or an address, he would follow the custom inaugurated early in the history of the Journal, of making copious extracts, and thus give to its readers an opportunity of examining materials which otherwise might not fall in their way.

Gray's contributions to the Journal comprise more than one thousand titles, without counting the memorial notices and the shorter obituary notes. In these notices he sums up in a few well-chosen words the contributions made to botany by his contemporaries. Even in the few instances in which he felt obliged to note with disapproval some of the work, he expressed himself with personal friendliness. The necrology, as it appeared from month to month, was a labor of love. All of the longer memorial notices are what it is the fashion now-a-days to call appreciations, and these are so happily phrased that it would seem as if the writer in many a case asked himself, "Would my friend, about whom I am now writing, make any change in this sketch?"

Gray on Darwinism.—In October, 1859, Darwin's epoch-making work, "The Origin of Species," was published. An early copy was sent to the editor of the Journal, Professor James D. Dana. This arrived in New Haven on December 21, but it was preceded by a personal letter which is of so much interest that it is here transcribed in full. It should be added that Dana was at this time in Europe where he was spending a year in the search for health after a serious nervous breakdown. In his absence the book was noticed by Gray as stated below. The letter is, as follows:

Down, Bromley, Kent.
Nov. 11th, 1859.

My dear Sir,

I have sent you a copy of my Book (as yet only an abstract) on the Origin of Species. I know too well that the conclusion, at which I have arrived, will horrify you, but you will, I believe and hope, give me credit for at least an honest search after the truth. I hope that you will read my Book, straight through; otherwise from the great condensation it will be unintelligible. Do not, I pray, think me so presumptuous as to hope to convert you; but if you can spare time to read it with care, and will then do what is far more important, keep the subject under my point

of view for some little time occasionally before your mind, I have hopes that you will agree that more can be said in favour of the mutability of species, than is at first apparent. It took me many long years before I wholly gave up the common view of the separate creation of each species. Believe me, with sincere respect and with cordial thanks for the many acts of scientific kindness which I have received from you,

My dear Sir,
Yours very sincerely,
CHARLES DARWIN.

In March, 1860 (**29**, 153), Gray published in the Journal an elaborate and cautious review of Darwin's work. He alluded to the absence of the chief editor of the Journal in the following words:

"The duty of reviewing this volume in the American Journal of Science would naturally devolve upon the principal editor whose wide observation and profound knowledge of various departments of natural history, as well as of geology, particularly qualify him for the task. But he has been obliged to lay aside his pen to seek in distant lands the entire repose from scientific labor so essential to the restoration of his health, a consummation devoutly to be wished and confidently to be expected. Interested as Mr. Dana would be in this volume, he could not be expected to accept its doctrine. Views so idealistic as those upon which his 'Thoughts upon Species' are grounded, will not harmonize readily with a doctrine so thoroughly naturalistic as that of Mr. Darwin . . . Between the doctrines of this volume and those of the great naturalist whose name adorns the title-page of this Journal [Mr. Agassiz] the widest divergence appears."

Gray then proceeds to contrast the two views of Darwin and Agassiz, "for this contrast brings out most prominently and sets in strongest light and shade the main features of the theory of the origination of species by means of Natural Selection." He then states both sides with great fairness, and proceeds:

"Who shall decide between such extreme views so ably maintained on either hand, and say how much truth there may be in each. The present reviewer has not the presumption to undertake such a task. Having no prepossession in favor of naturalistic theories, but struck with the eminent ability of Mr. Darwin's work, and charmed with its fairness, our humbler duty will be performed if, laying aside prejudice as much as we can,

we shall succeed in giving a fair account of its method and argument, offering by the way a few suggestions such as might occur to any naturalist of an inquiring mind. An editorial character for this article must in justice be disclaimed. The plural pronoun is employed not to give editorial weight, but to avoid even the appearance of egotism and also the circumlocution which attends a rigorous adherence to the impersonal style."

In this review he moves slowly and thoughtfully, but not timidly, over the new paths. There is no clear indication in the review that he has yet made up his mind as to the validity of Darwin's hypothesis. But, in a second article appearing in the Journal for September of the same year (**30**, 226), under the title "Discussion between two readers of Darwin's treatise on the origin of species upon its natural theology" Gray plainly begins to incline to take a very favorable view of the Darwinian theory, and makes use of the following ingenious illustration to show that it is not inconsistent with theistic design. A few paragraphs here quoted show the felicity of his style in a controverted matter:

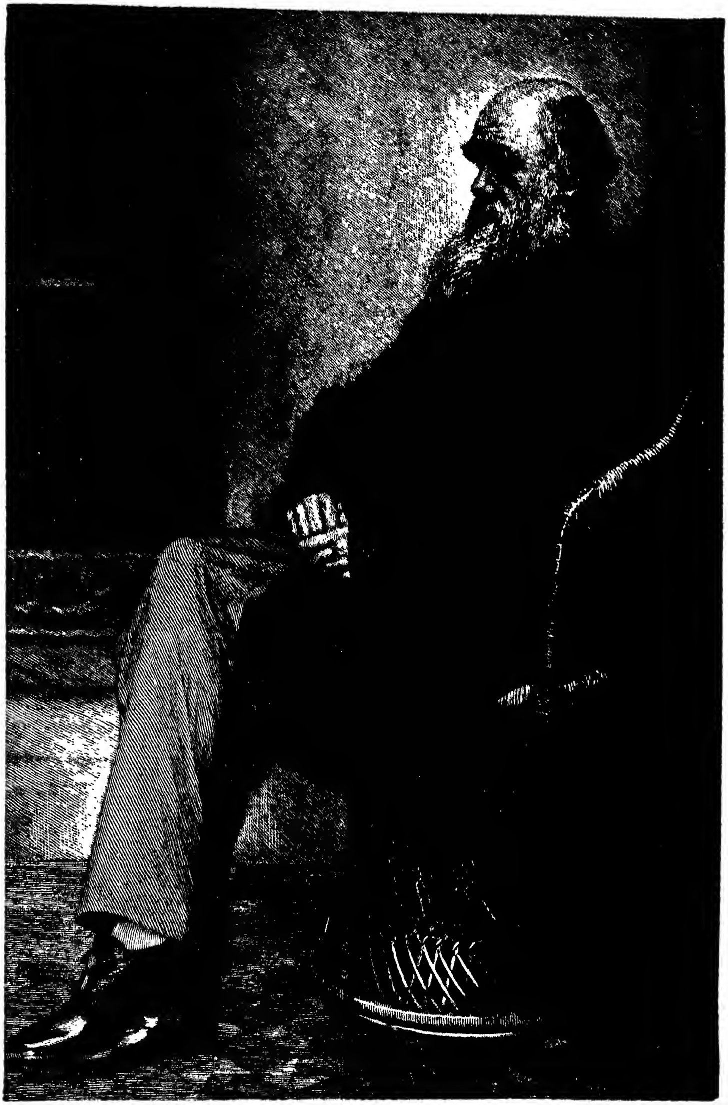
"Recall a woman of a past generation and show her a web of cloth; ask her how it was made, and she will say that the wool or cotton was carded, spun, and woven by hand. When you tell her it was not made by manual labor, that probably no hands have touched the materials throughout the process, it is possible that she might at first regard your statement as tantamount to the assertion that the cloth was made without design. If she did, she would not credit your statement. If you patiently explained to her the theory of carding-machines, spinning-jennies, and power-looms, would her reception of your explanation weaken her conviction that the cloth was the result of design? It is certain that she would believe in design as firmly as before, and that this belief would be attended by a higher conception and reverent admiration of a wisdom, skill, and power greatly beyond anything she had previously conceived possible."

By this review Gray disarmed hostility to such an extent that some persons who had been antagonistic to Darwinism accepted it with only slight reservation. It may be fairly claimed that the Journal bore a leading part in influencing the views of naturalists in America in regard to the Darwinian theory.

Dr. Gray soon put the Darwinian hypothesis to a severe test. In the Journal for 1840 he had called attention to the remarkable similarity which exists between the flora of Japan and a part of the temperate portion of North America. The first notice of this subject by him occurs in a short review of Dr. Zuccarini's "Flora Japonica," a work based on material furnished by Dr. Siebold, who had long lived in Japan. In this review (**39**, 175, 1840), he enumerates certain plants common to the two regions, and says, "It is interesting to remark how many of our characteristic genera are reproduced in Japan, not to speak of striking analogous forms." In a subsequent paper (**28**, 187, 1859), he recurs to this subject, and, after alluding to geological data furnished by J. D. Dana, he says:

"I cannot resist the conclusion that the extant vegetable kingdom has a long and eventful history, and that the explanation of apparent anomalies in the geographical distribution of species may be found in the various and prolonged climatic or other vicissitudes to which they have been subject in earlier times; that the occurrence of certain species, formerly supposed to be peculiar to North America, in a remote or antipodal region, affords in itself no presumption that they were originated there, and that interchange of plants between eastern North America and eastern Asia is explicable upon the most natural and generally received hypothesis (or at least offers no greater difficulty than does the arctic flora, the general homogeneousness of which round the world has always been thought compatible with local origin of the species) and is perhaps not more extensive than might be expected under the circumstances. That the interchange has mainly taken place in high northern latitudes, and that the isothermal lines have in earlier times turned northward on our eastern and southward on our northwest coast, as they do now, are points which go far towards explaining why eastern North America, rather than Oregon and California, has been mainly concerned in this interchange, and why the temperate interchange, even with Europe, has principally taken place through Asia."

This paper was communicated in 1859, on the eve of the publication of Darwin's "Origin of Species." At a later date he applied the Darwinian theory to the possible solution of the problem, and came to the conclusion that the two floras had a common origin in the Arctic



Ch. Darwin

From "Life and Letters of Charles Darwin" by Francis Darwin.

zone, during the Tertiary period, or the Cretaceous which preceded it, and the descendants had made their way down different lines toward the south, the species varying under different climatic conditions, and thus exhibiting similarity but not absolute identity of form. Before the American Association for the Advancement of Science, in his Presidential address, in 1872, he used the following language:

"'According to these views, as regards plants at least, the adaptation to successive times and changed conditions has been maintained, not by absolute renewals, but by gradual modifications. I, for one, cannot doubt that the present existing species are the lineal successors of those that garnished the earth in the old time before them, and that they were as well adapted to their surroundings then, as those which flourish and bloom around us are to their conditions now. Order and exquisite adaptation did not wait for man's coming, nor were they ever stereotyped. Organic Nature—by which I mean the system and totality of living things, and their adaptation to each other and to the world—with all its apparent and indeed real stability, should be likened, not to the ocean, which varies only by tidal oscillations from a fixed level to which it is always returning, but rather to a river, so vast that we can neither discern its shores nor reach its sources, whose onward flow is not less actual because too slow to be observed by the ephemeræ which hover over its surface, or are borne upon its bosom.''

Gray's active interest in the Journal continued until the very end of his life. There were many critical notices from his pen in 1887. His last contribution to its pages was the botanical necrology, which appeared posthumously in volume 35, of the third series (1888). His connection with the Journal covered, therefore, a period of more than a half a century of its life.[2]

The changes that were wrought in botany by the application of Darwinism were far reaching. Attempts were promptly made to reconstruct the system of botanical classification on the basis of descent. The more successful of these endeavors met with welcome, and now form the groundwork of arrangement of families, genera, and species, in the Herbaria in this country, in the manuals of descriptive botany, and in the text-books of higher grade. This overturn did not take place until after

Gray's death, although he foresaw that the revolution was impending.

One of the most obvious changes was that which gave a high degree of prominence in American school treatises to the study of the lower instead of the higher or flowering plants, these latter being treated merely as members in a long series, and with scant consideration. But of late years, there has been a renewed popular interest in the phænogamia, leading to a more thorough investigation of local floras, and also to the examination of the relations of plants to their surroundings. The results of a large part of this technical work are published in strictly botanical periodicals and now-a-days seldom find a place in the pages of a general journal of science.

Cryptogamic Botany in the Journal since 1846.

In glancing rapidly at the First Series it has been seen that a fair share of attention was early paid by the Journal to the flowerless plants. So far as the means and methods of the time permitted, the ferns, mosses, lichens, and the larger algæ and fungi of America were studied assiduously and important results were published, chiefly on the side of systematic botany.

The Second Series comprises the years between 1846 and 1871. In this series one finds that the range of cryptogamic botany is much widened. Besides interesting book notices relative to these plants, there are a good many papers on the larger fungi, on the algæ, and mosses. Here are contributions by Curtis, by Ravenel, by Bailey, and by Sullivant. The lichens are treated of in detail by Tuckerman, and there are some excellent translations by Dr. Engelmann of papers by Alexander Braun. Some of the destructive fungi are considered, as might well be the case in the period of the potato famine. It is in these years that one first finds the name of Daniel Cady Eaton, who later had so much to do with developing an interest in the subject of ferns in this country. He was a frequent contributor of critical notices.

Cryptogamic Botany, as it is now understood, is a comparatively modern branch of science. The appli-

ances and the methods for investigating the more obscure groups, and especially for revealing the successive stages of their development, were unsatisfactory until the latter half of the last century. Gray recognized this condition of affairs, and appreciated the importance of the new methods and the better appliances. Therefore he viewed with satisfaction the pursuit of these studies abroad by one of his students and assistants, William G. Farlow. Dr. Farlow carried to his studies under DeBary and others unusual powers of observation and great industry. He speedily became an accomplished investigator in cryptogamic botany and enriched the science by notable discoveries, one of which to-day bears his name in botanical literature. On his return to the United States, Farlow entered at once upon a successful career as an inspiring teacher and a fruitful investigator. He became a frequent contributor to the Journal, keeping its readers in touch with the more important additions to cryptogamic botany. He had wisely chosen to deal with the whole field, and consequently he has been able to preserve a better perspective than is kept by the extreme specialist. The greater number of cryptogamic botanists in this country have been under Professor Farlow's instruction.

Systematic and Geographical Botany of Late Years.

The usefulness of the Journal in descriptive systematic botany of phanerogams is shown not only by its acceptance of the leading features of DeCandolle's Phytography, where very exact methods are inculcated, but by the very numerous contributions by Sereno Watson and others at the Harvard University Herbarium, as well as from private systematists. It is in the pages of the Journal that one finds the record of much of the critical work of Tuckerman and of Engelmann, in interesting Phanerogamia. Of late years the Journal has had the privilege of publishing a good deal of the careful work of Theo Holm, in the difficult groups of Cyperaceæ, and also his admirable studies in the morphology and the anatomy of certain interesting plants of higher orders.

Attention was called, in passing, to Gray's deep inter-

est in geographical botany. In this important branch, besides his contributions, one finds, among many others, such papers as LeConte's "Flora of the Coast Islands of California in Relation to Recent Changes of Physical Geography" (**34**, 457, 1887), and Sargent's "Forests of Central Nevada" (**17**, 417, 1879). Examination reveals a surprising number of communications which bear indirectly upon this subject.

Paleontological Botany.

When the Journal began its career, the subject of fossil plants was very obscure. Brongniart's papers, especially the Journal translations, enabled the students in America to undertake the investigation of such fossils and the results were to a considerable extent published in the Journal. Since the subject belongs as much to geology as to botany, it finds its appropriate home in the pages of the Journal. The recent papers on this topic show how great has been the advance in methods and results since the early days of the Journal's century. Under the care of George R. Wieland, the communications and the bibliographical notices of paleontological treatises show the progress which he and others are making in this attractive field.

Economic Botany, Plant Physiology, etc.

At the outset, the Journal, as we have seen, devoted much attention to certain phases of economic botany, and, even down to the present, it has maintained its hold upon the subject. The correspondence of Jerome Nicklès from 1853 to 1867 brought before its readers a vast number of valuable items which would not in any other way have been known to them. And the Journal dealt wisely with the scientific side of agriculture, under the hands of S. W. Johnson and J. H. Gilbert, and others, placing it on its proper basis. This work was supplemented by Norton's remarkable work in the chemistry of certain plants, the oat, for example, and certain plant-products. In fact it might be possible to construct from the pages of the Journal a fair synopsis of the important principles of agronomy.

Physiology has been represented not only by the studies which had been inaugurated and stimulated by the Darwinian theory, such as the cross-fertilization and the close-fertilization of plants, plant-movements, and the like, but there have been a good many special communications, such as Dandeno on toxicity, Plowman on electrical relations, and ionization, and W. P. Wilson on respiration.

There are many broad philosophical questions which have found an appropriate home in the Journal, such as "The Plant-individual in its relation to the species" (Alexander Braun, **19**, 297, 1855; **20**, 181, 1855), and "The analogy between the mode of reproduction in plants and the alternation of generations observed in some radiata" (J. D. Dana, **10**, 341, 1850). Akin to these are many of the reflections which one finds scattered throughout the pages of the Journal, frequently in minor book-notices. As might be expected, some attention has been paid to the very special branch of botany which is strictly called medical. For example, early in its history, the Journal published a long treatise by Dr. William Tully (**2**, 45, 1820), on the ergot of rye. This is considered from a structural as well as from a medical point of view and is decidedly ahead of the time in which it was written. There are a few references to vegetable poisons, and there is a fascinating account of the effect of the common white ash on the activities of the rattlesnake. In short it may be said that the editor did much towards making the Journal readable as well as strictly scientific.

The list of reviewers who have been permitted to use the pages of the Journal for notices of botanical and allied books in recent years is pretty long. One finds the initials of Wesley R. Coe, George P. Clinton, Arthur L. Dean, Alexander W. Evans, William G. Farlow, George L. Goodale, Arthur H. Graves, Herbert E. Gregory, Lafayette B. Mendel, Leo F. Rettger, Benjamin L. Robinson, George R. Wieland, and others.

At the present time, in the biological sciences, as in every department of thought, there is great specialization, and each specialty demands its own private organ of

publication. Naturally this has led to a falling off in the botanical communications to the Journal, but it cannot be forgotten that the history of North American Botany has been largely recorded in its pages.

Notes.

[1] Scientific Papers of Asa Gray. Selected by Charles Sprague Sargent. Two volumes, Boston, 1889 (see notice in vol. 38, 419, 1889).

[2] A notice of Gray's life and works is given by his life-long friend, J. D. Dana, in the Journal in 1888 (35, 181-203).

Erratum for "A CENTURY OF SCIENCE."

Page 51, line 30, and page 54, line 2. For "George W. Goodale" read George L. Goodale."